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THE MONTHLY

JOURNAL OF SCIENCE.

MARCH, 1879.

I. THE KEYS OF DEATH.

IS strychnin, or prussic acid, or arsenic a poison? The answer is ready. What is a poison? The oracles are dumb. We have in these days almost ceased playing at the game of definitions. We no longer ask what is gravitation, or motion, or light, feeling that our answers to such questions can only bewilder. Definitions of life are still gravely asked for, and as gravely propounded. But they, too, will soon be given up, and neither Science nor daily life will find cause to regret them. As concerns a satisfactory definition of poison the case is very similar. Two attempts have, however, been lately made to supply this deficiency. Certain physiologists, speaking in the interest of a certain philanthropic movement, contend that the idea of poison is purely qualitative, and that whatever is poisonous in large doses is poisonous also in the very smallest proportions, the only difference being that the injury done may escape observation unless the dose is often repeated. We cannot for a moment accept this view. We maintain that it is perfectly unchemical to ignore the ideas of quantity and concentration. It would be easy to give numerous instances where—if the reagents employed are less in proportion, or weaker than the required standard—we obtain not a smaller quantity of the product sought for, but a substance totally different, or, in certain cases, no action at all. Or if we, coming at once to the mark, fix our attention upon the higher animals, we find that for them all oxygen is the first necessary of life. If deprived of it for but a few minutes they inevitably perish. Yet Davy proved

that animals placed in an atmosphere of pure oxygen are thrown into a morbid state, which, if the experiment is sufficiently prolonged, terminates fatally. Here, therefore, we see that dilution converts oxygen from a poison to a necessary of life. Or take sulphuric acid. If swallowed in a concentrated state it destroys all parts of the system which it touches. Dilute it largely with water, and it becomes a pleasant summer beverage of tonic properties. Open an animal which has swallowed such dilute acid, and examine the œsophagus and stomach with the most powerful microscope, and you will find no trace of those phenomena caused by the undilute acid. The results differ not in extent, not in degree, but in kind. These instances, which might be indefinitely multiplied, prove that the idea of poison is not essentially qualitative.

Let us now examine the second notion which has been brought forward to help us to a precise definition of poisons. It is alleged that whilst articles of food undergo decomposition in the stomach, poisons are not thus decomposed, but are absorbed unchanged, exist in the tissues unchanged, and in cases of recovery are finally expelled still in the same state in which they were first ingested. This explanation sins in the first place by implying that all articles taken into the system must be either food or poison. But, further, will chromic acid undergo no change if taken internally? How will the chlorides of arsenic or of cyanogen behave? There is again a liquid, neither rare nor entirely strange to chemists, which agrees with the features here ascribed to poisons as well as does alcohol. This liquid, if swallowed either in large or small doses, remains undecomposed in the stomach, is taken up by the absorbents, and enters the blood and the tissues, still without having undergone decomposition, and is finally eliminated as such in the urine and the perspiration. If, therefore, the above definition of poison be correct, this liquid is a poison; but if it is found by common experience harmless and necessary, what of the definition? The liquid in question is water, which has never been proved to suffer decomposition in the system, and which therefore *ought* to be a poison!

Leaving these discussions for more profitable considerations, we find included under the general name of "poisons" two classes of bodies having little in common save their injurious effects upon the living organism into which they are introduced. We have, on the one hand, certain ferments—"germs," or by whatever other name they may be

known—which, when absorbed by an animal, bring on such diseases as rabies, small-pox, cholera, dysentery, typhoid, and the whole class of so-called zymotic affections. On the other hand, there are the true poisons, such as arsenic, strychnin, aconitin, hydrocyanic acid, &c. The substances of this latter class are well-defined chemical individuals, and even when, as is the case with all the most deadly, they are of vegetable or animal origin,—an awkward fact for the charlatans who guarantee the "purely vegetable" nature of their life-pills,—their efficacy has no connection with vitality. Poisons of this class when introduced into the system set up morbid action almost immediately, and if the dose be sufficient, and if no remedial measures are adopted, the symptoms go on increasing in violence without intermission until death ensues. A characteristic feature is that their noxious power may be decreased, or even totally extinguished, by dilution. If a pound of arsenic, or even of aconitin, were dissolved in the head-waters of the Thames, not the least danger would result to any of the thousands of people who drink Thames water as supplied to certain districts of London. The ferments, on the contrary, are not definite chemical principles, capable of being isolated, of entering into combination with other bodies, and of being separated again. So far as we know they are living organisms of low type, belonging to that "debatable land" that lies on the margins of the animal and the vegetable kingdoms, and they retain their virtues, or vices, only so long as their vitality endures. If swallowed, or otherwise introduced into the system, no evil effect is perceived for some time. There is a so-called "period of incubation" which in the case of *rabies* may extend over months, or even years, before the symptoms of disease make their appearance. Another characteristic is that the poison is multiplied in acting. If we kill an animal by the administration of a dose of strychnin or of arsenic, we cannot obtain from its body any more poisonous matter than we put in; but if we inoculate an animal with a ferment-poison we find in some cases its blood, in others its secretions and excretions, capable of reproducing the same disease in other animals. The ferment grows and fructifies in its victim, just as does wheat in a field. The ferments, too, lose none of their deadly power by dilution. If, returning to our former example, we placed a quantity of the excretions of a cholera patient or of a victim of typhoid fever in the head-waters of the Thames, there would be great danger to persons sup-

plied with such water in London. By this dilution-test it has been established that the poison of the cobra—and in all probability of other venomous serpents—is not a ferment, but a true chemical principle. Hence all anti-zymotics—such as benzoic, salicylic, carbolic, and cresylic acids—are necessarily impotent in the treatment of snake-bites.

A distinction was formerly attempted to be drawn between poisons active when swallowed and others supposed to be potent only when introduced directly into the blood. To this latter class the venomous secretions of snakes were supposed to belong. This is now known to be an error. The poison of the cobra, if in a sufficient dose, is as deadly when swallowed, or applied to a mucous membrane, as if conveyed into the blood. Some of the disease ferments are certainly active when swallowed, and, although there may be doubt concerning the poisons of rabies, of carbuncle, and of yellow fever, discretion is in all such cases the better part of valour.

The common sense of mankind has always deemed poison to be specific, like food, but in an even more decided manner. It has concluded from observation that a substance, harmful or even deadly to certain animal species, may be to others perfectly innocent. Certain modern writers, undertaking somewhat prematurely to explain the respective nature of foods and of poisons, have more or less explicitly denied this view. In the very teeth of experience they have asserted that any given substance is poisonous or otherwise in virtue of its own chemical attributes, no regard being had to the nature of the animal into whose system it is to be introduced. This opinion is no less contrary to general analogy than to direct observation. We bring together certain reagents in a glass vessel, and we obtain a given result; but if we perform the same experiment within the stomach or in the tissues of an animal, the result will in many cases be seriously modified. Nor can we even from an experiment performed upon one species conclude with certainty what will happen in the case of another. Nay, even within the limits of one and the same species we encounter in this respect more or less diversity. This is notably the case with the poisons of unwholesome mushrooms, and of certain species of fish and mollusks. It is no uncommon thing for a number of persons to partake jointly of such esculents, and while some of the party sicken, even fatally, the remainder are not in the slightest degree inconvenienced. Such idiosyncracies cling even to the life-

less remains of animals. If different samples of wool are prepared in the same manner, and dyed together in the same pan, some of them will not only be darker in shade than others, but even a difference in tone will be occasionally detected.

Coming more directly to the point, we find certain animals feeding heartily upon plants which to other species, and especially to man, are decidedly poisonous. Snails and slugs devour the leaves of the foxglove as safely, and relatively in as large quantities, as a cow consumes grass, and they are also fond of the Solanaceæ, including the belladonna. The caterpillars of the following Lepidoptera feed upon poisonous plants:—*Gonopteryx rhamni* upon *Rhamnus catharticus; Thats polyxena* upon species of *Aristolochia; Danais Archippus* and *Chrysippus* upon various Asclepiads; *Deilephila Galii, Nicea,* and *Euphorbiæ* upon species of *Euphorbia; Chœrocampa Nerii* upon the oleander; *Sphinx ligustri* upon privet and laurel; *Noctua baja* upon belladonna; *Polia cappa* upon staves-acre; *Heliothisa armigera* upon tobacco; and *Chrysoptera moneta* upon monkshood. Turning to the Vertebrates we find that domestic fowls eat the foxglove; the hornbill consumes with perfect impunity the deadly fruit of species of *Strychnos;* whilst the hare and the rabbit nibble the belladonna and gnaw the bark of the mezereon.

We are by no means attempting to furnish a catalogue of species which feed upon plants noxious or fatal to man, but merely give a few of the best-established instances in proof that what is poisonous to one species may be to another perfectly harmless. There are doubtless poisons which are fatal to all. No animal save the serpent which secretes the poison has been found able to withstand the venom of the cobra.

In looking over the instances above given, and seeking their explanation, we may suppose that the deleterious principles of the plants mentioned may never be really absorbed. Like gum and cellulose, they may simply pass along the intestinal canal and be rejected with the solid excreta without ever entering the system at all. But this hypothesis, even if demonstrated, would fail to solve the question. We should still have to ask, why are these substances absorbed in one animal and in others harmlessly rejected? Further, in many cases we know that the poison is absorbed. When the common viper bites a hedgehog we know that the poison enters the blood. What renders it inert, as witnessed in

the experiments of Dr. Lenz? Morphia if swallowed by certain apes enters their circulation, and can be readily detected in their urine in large quantities. Why is it in them eliminated without occasioning harm? We note, further, an important distinction: the exemption which any animal enjoys from the effects of some particular poison may be merely relative, as compared with the action of the same drug on man, or it may be absolute. To poison a hedgehog with cantharides would be as hopeful a task as to poison a Neapolitan with macaroni. But when it is loosely said that a horse cannot be poisoned with arsenic, or an ape with morphia, it is meant merely that these animals can endure larger doses than can we without perceptible harm.

There are cases in which copper—a well-known irritant poison—is not merely absorbed, but assimilated, and appears even to be an essential of life. Harless detected copper in the blood and in the liver of certain mollusks, especially cephalopods and Ascidiæ. His experiments show that it "stands in an essential relation to the blood-corpuscules." Indeed in the blood of the cephalopods there has been detected an organic copper compound, known as hæmocyanin, which fulfils the function due in the higher animals to hæmoglobin. Von Bibra detected copper on certain crustaceans, such as *Cancer pagurus*, and found that it occurred in an inverse ratio to the iron which it more or less completely replaced. Dr. Genth quantitatively determined the copper in the blood of the *Limulus*, a crab found on the coasts of North America. The blood of the healthy females, taken just before laying their eggs, was of a deep blue, and when dried the incinerated residue contained 0·295 per cent oxide of copper, and but a mere trace of oxide of iron. How strangely would the appearance of mankind be altered had we, like the *Limulus*, copper instead of iron in our blood! A rich azure would mantle on the cheek of beauty; we should extol in song the charms of sea-green tresses, and liken the lips we love not to coral, but to malachite. Perhaps the dealers in cupriferous pickles and preserved fruits and vegetables are striving to bring about this consummation.

Copper in an organic compound named touracin has been detected by Prof. Church in the feathers of the touraco. That it must consequently be normally present in the blood of this bird follows of necessity.

We may conceive it possible, both as regards disease-ferments and poisons properly so-called, that strains o

beings indifferent to their action may gradually be formed by a process of natural selection. Scarcely any other hypothesis will explain the facts that certain races of men resist disease-poisons to which others succumb, and that an epidemic on its first visit to any country commits such frightful ravages. A striking instance of this was shown by the late spread of the measles in the Fiji Islands. It is exceedingly probable that the resistance of the slug to digitalin and atropin may have been produced in the same manner as the negro's non-susceptibility to yellow fever. Indeed one of the most curious facts connected with poisons is that individual men, and doubtless animals, by beginning with small doses, can gradually habituate themselves even to the most deadly compounds. The legend of Mithridates contains a basis of solid truth. Without depending upon disputed cases, such as that of the arsenic-eaters of Styria, the history of opium-eating gives abundant evidence of this acquired immunity. We at one time knew a gentleman, connected with the Inland Revenue in a town of the West Riding, who was no more affected by a wine-glassful of laudanum than is an ordinary man by the same quantity of sherry. Very similar is the case with tobacco, which, though it produces severe constitutional disturbance in persons unaccustomed to its use, is afterwards borne without the slightest inconvenience. The question may, indeed, be raised whether a similar immunity from snake-poison might not be acquired by habituating the system to doses at first infinitesimal, and gradually becoming larger and larger. Our knowledge concerning the physiological action of such poisons is as yet exceedingly rudimentary, and it is quite possible that there might be some serious objection to such inoculation. Still we should recommend this idea to the experimental notice of the medical profession in countries such as India and Australia, where venomous snakes abound, and where biological research has not yet been fettered at the bidding of a hysterical humanitarianism.

We mentioned above that on the entrance into the system of a true poison morbid phenomena at once make their appearance. To this rule there is an exception, *i.e.*, when the poison is administered in successive doses too small to produce the usual alarming symptoms, but too large or too frequent to allow the system to become habituated to their effects. Certain poisons indeed, such as lead, accumulate in the system, whilst others—colchicin being perhaps the best example—are cumulative in their action. We have

thus a group of facts of which it is hard to speak without denying the facts we have just mentioned on the acquired toleration of poisons. We have, indeed, here to deal with very complicated effects. We know that the drunkard or the opiumist can swallow, without apparent result, doses of alcohol, of methyl- and amyl-compounds, or of laudanum, which would be rapidly fatal to a person "unseasoned." Still we know that this immunity is bought at the expense of his general health.

These considerations lead us to the question of "slow poisoning." Popular tradition and imaginative literature often introduce us to a very interesting villain, who gives his victims a dose calculated with such marvellous precision as—whilst producing no immediate action—to bring on inevitable death after the lapse of a certain number of months. Much of this may be safely pronounced impossible. Nevertheless a few ugly instances have occurred lately which warn us that in this department, as in many others, modern Science has not yet thrown an electric light into all the dark hiding-places of Nature. We know some of the details of the case of a retired Indian officer, who died not long ago at a town in one of the Eastern counties, and who displayed symptoms utterly irreconcilable with any recognised disease, and scarcely intelligible on any other hypothesis than that of an unknown poison administered previous to his return home.

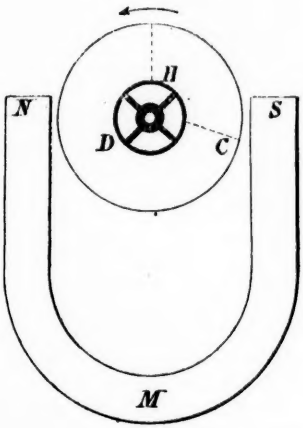
We have thus taken a hasty and necessarily superficia survey of a subject which presents to the chemist and the biologist numbers of unsolved problems—problems not merely of high scientific interest, but of the greatest practical importance. This tempting harvest-field is now, however, closed to Englishmen. Our laws, which with inimitable logic once deemed that "in a liberal construction copper is tin," now see "vivisection" where there is no "section" at all.

II. A CURIOUS THERMO-MAGNETIC MOTOR.

By Profs. EDWIN J. HOUSTON and ELIHU THOMSON.

DURING investigations by the authors, concerning the increase in the coercitive force of steel by changes of temperature, the following curious thermo-magnetic motor was devised. This motor, though devoid of practical value, will, no doubt, be of sufficient scientific interest to warrant a short description :—

In the figure, a disc or ring of thin steel, D, is mounted on an axis, so as to be quite free to move. The edges of the



wheel are placed opposite the poles, H and S, of a magnet. In this position the wheel of course becomes magnetised by induction.

If, now, any section of the wheel, as H, be sufficiently heated, the disk will move in the direction shown by the arrow. The cause of this motion is as follows :—The section H, when heated, has its coercitive force thereby increased, and being less powerfully magnetised by the induction of the pole S than the portion C, immediately adjacent to it, the attraction exerted by the pole S on the latter portion is thereby sufficient to cause a movement of the disk in the direction shown by the arrow. If a constant

source of heat be placed at H, a slow rotation in the direction shown is maintained.

To ensure success the disk must be sufficiently thin as to prevent its acquiring a uniform temperature. If the source of heat be at the same time applied at diametrically opposite portions of the disk, as at H and D, adjacent to the poles, the same effect will be produced. Since the amount of heat expended in producing motion of the disk is so enormous when compared with the force developed, it will be readily understood that this motor is of no value as such, but must be regarded as an interesting example of the interconvertibility of force.—*Journal of the Franklin Institute.*

III. THE HEAT OF THE COMSTOCK MINES.*

By Prof. JOHN A. CHURCH, E.M., Columbus, Ohio.

ONE of the most striking phenomena connected with the mines on the Comstock lode is the extreme heat encountered in the lower levels. This heat proceeds from the rock, which maintains constantly a temperature very much higher than the average of the atmosphere in Nevada.

The heat of these mines is a matter of more than usual interest, for they are the only hot ones now worked in the United States, and both in the present temperature encountered and in the increase which is to be expected as greater depths are reached, they appear to surpass any foreign mines of which we have a record.

The rock in the lower levels of the Comstock mines appears to have a pretty uniform temperature of 130° F.

The low conductivity of minerals to heat forbids the supposition that a rock of 130° F. temperature can lose heat

* Abstract of a Paper read at the Chattanooga Meeting of the American Institute of Mining Engineers, May, 1878, by permission of Lieut. Geo. M. Wheeler, Corps of Engineers, U.S.A., in charge of U.S. Geographical and Geological Surveys, west of the 100th meridian.

sensibly to any depth in the course of twenty-four hours. The shallow holes which were made use of always lay in new ground, and exhibited results which may be accepted with as much confidence as if they were 20 feet or more deep. Very often they were in ground which had been exposed only one or two hours, having been sunk immediately after a blast which threw off 4 or 5 feet of the rock. The surface which was thus thrown down had not been exposed more than twenty-four hours. The high temperature and small flow of air in the heading forbid the supposition that any sensible diminution of heat could have taken place at the bottom of the drill-hole under such circumstances. The surface of the rock exposed to the air of the drift was found to be about 123° F., the experiment being made near the "header" or end of the drift. The air itself was found to show considerable uniformity when its temperature was taken under circumstances that were at all similar. In freshly opened ground it varied from 108° to 116° F., and higher temperatures are reported at various points, reaching in fact as high as 123° F. in the 1900 level of the Gould and Curry.

The temperature of the air is subject to more fluctuations than that of the rock, for the simple reason that it is artificially supplied to the mine, and varies according to the distance to which it is carried, the quantity, velocity in the pipe, and its initial temperature. All of these elements of the problem vary within wide limits. The initial temperature of the air which supplies a particular drift will, for instance, depend upon whether it is drawn from the surface, the bottom of a shaft, where it is often cooler than above ground, or from some old air-way, where it has had time and opportunity to take up heat.

In the Comstock mines it is the custom, without exception, to blow the air through galvanised iron pipes, the diameter of which is usually from 8 to 20 inches. The size most used is 11 inches in diameter, and the usual amount of air blown is about 700 cubic feet per minute, this being the supply for two to six or more men, working in one or two "headers."

In most cases the air is not sent down from the surface, but taken from some point in the incline or at the bottom of the shaft. Its temperature may be assumed at about 80° or 90° F. in summer, though it is sometimes higher than this. Its velocity in the air-pipe is not very far from 1000 feet per minute. From these data it will be seen that about 15 or 20 degrees of heat are added to the air, in a period of time

varying from half a minute to two minutes. The iron of the pipe is so thin and its conductivity so great that there is practically a slender current of air moving through a body of hotter air.

The iron receives heat both by immersion in the hot air and by direct radiation from the still hotter walls. The currents confined in it must be thrown against its sides by eddies, and the air is thus made to absorb heat by contact as well as by the transmission of heat-rays through it.

Drifts that do not exceed 200 or 300 feet in length are *usually* not above 110° or 112° F. in temperature, and more often they are below this. But when the length increases to 1200 and 1500 feet the temperature may rise to 116° F. without any other change in the circumstances.

So far as the author's personal experience goes, the latter temperature has not been exceeded in any drift into which a good current of air is blown. By a "good current" he means one of not less than 700 or 1000 cubic feet a minute. Still he has no hesitation in asserting his confidence in the higher temperatures which others have sometimes obtained. The view which he takes of the phenomenon and its cause admits of such exceptional heat at particular points as a rational consequence of the forces at work. But he regards them as exceptional, and believes the average temperature of those drifts which are considered to be distinctively "hot" is usually not above 108° to 112° F., though rising to 116° F., when they are very long.

These limits are, however, not in the least degree true of the water which enters the drifts from the country rock, and also from the lode rocks. That approaches more nearly 150° F. The large body of water which has filled the Savage and Hale and Norcross mines for more than a year, and from which it is safe to say a million tons of water have been pumped within twelve months, gave a temperature of 154° F. Even after being pumped to the surface through an iron pipe exposed, in the shaft of the Hale and Norcross, to a descending current of fresh air for more than 1000 feet, and then flowing for 100 or 200 feet through an open sluice in a drain-tunnel which discharges into a measuring-box, the water in this box was found to have a temperature of no less than 145° F.

But the water varies in temperature in different parts of the lode, like the rock and the air. In the East cross-cut 2000 feet level, of the Crown Point Mine, which is noted for its extreme heat, the water, after flowing for nearly 150 feet over the bottom of the drift, was found to have a tempera-

ture of 157° F. On the contrary, the water in other places is much less hot, but it is as a rule always hotter than the air, and in many cases it appears to be hotter than the rock is found to be, except in especially hot spots.

The East cross-cut of the Crown Point 2000 feet level, which was temporarily abandoned and boarded up on account of the heat, gave an *air* temperature of 150° F., the thermometer being thrust through a crack in the boarding. At the head of this cross-cut the heat was proved to be higher than this.

Another hot spot is in the Imperial Consolidated Mine. In this mine the Black Dyke splits, sending a shoot off to the north-east, and a drift has been run on the 2000 feet level, along the eastern side of this branch dyke.

This proved to be a very hot spot indeed. Rock, air, and water were all so much above the usual limits of temperature, even in these hot mines, that the work of cutting the drift must have been extremely severe.

The Belcher south incline has a hot belt of rock, quite narrow, a short distance above the nineteen hundred station, and similar hot places are found in most of the mines.

The author inclines to the opinion that, as a general rule, these hot areas lie in belts, and are not irregular or promiscuously placed in the mass of East country rock. Where this seems to be disproved by the distance run in the superheated rock, it will, he thinks, probably be found that the drift, or incline, and the hot belt have the same direction.

Hot belts are also found at the contact of the diorite and propylite in the Virginia mines. The diorite is itself in active decomposition, and mines which have carried drifts in or near it are very hot. The Julia has explored a quartz seam, which appears to lie entirely in the diorite, and this has proved to be one of the hot belts.

This apparent concentration of the heat in the line of contact of two rocks is not supposed to be due to any thermal or electro-thermal action, but to depend merely upon the fact that in this neighbourhood the ground is more broken and the surfaces of the rock increased. These conditions are obviously favourable to the action of atmospheric waters.

Belts of excessively hot ground are not the only noticeable phenomena in these mines. More remarkable still are the belts of unusually *cold* rock: these are fewer in number than the hot belts, but they are strongly marked; they are always wet, and the water that drips through the crevices

of the shattered rock that composes them is noticeably cold to the touch, and cools down the air of the drift. Such a wet, cold belt of rock exists on the 800 feet level of the Justice Mine, and there is a very decided change of temperature in passing from one to the other side of it. Water drips from the rock in numerous places in these as in most mines, and that usually it is hot, or at least warm.

Other cold belts are found in the mines which are not so cool as that in the Justice, but are perceptibly cooler than the rock at a short distance from them. They complete a well-linked chain of heat phenomena, extending from rocks that are sensibly cold to the touch, and may not have a temperature above 50° or 60° F., through rocks that have the average atmospheric temperature, and those which are as hot as surface rocks ever become in Nevada, to those which have a temperature of 157° F.

Finally, in the chain of testimony relating to this phenomenon is to be noted the condition of the rock. Wet places have been spoken of, but the rock cannot be considered as generally wet. There are water-ways, and many of them appear to reach the surface, but they are of limited breadth, like the belts of hot rock. This water is usually hot, but sometimes cool or tepid.

Very often, usually in fact, the rock is perfectly dry, though very hot: that is the case in all the mines. Wet rock is the exception, and dry rock the rule, through the whole lode. In the drifts cut through this hot, dry rock, the walls of the freshly exposed surfaces are painful to the hand, and the air is often filled with dust. The rock is both hard and tough, but, in spite of its strength, it gives an impression of fine porosity to the touch, due probably to its trachytic character. It often has the odour of clay, but not always. It may be slightly adherent, or the impression of dryness upon the tongue may be due to its heat, or to the fine dust which covers every fragment.

The heat in the Comstock and other mines similarly situated is quite generally spoken of as the feeble remnant of a temperature that once reached the point of rock fusion, but the facts observed have led the author to refer the high temperatures encountered in the mines not to the internal heat of the earth, nor to the residual heat of the rocks, which were once melted, but to chemical action now maintained in the erupted rocks.

This action is not a combustion, for the oxidisable minerals in the lode and its accompanying rocks, the metallic sulphides, are little altered. In fact, the total quantity of

pyrite and other sulphides is not large for the neighbourhood of a mineral lode, but on the contrary, strikingly small, and not sufficient to maintain the heat of the rocks and water, except under circumstances of unusually rapid oxidation. That no metallic oxidation of any moment goes on in these rocks is susceptible of proof. The metallic sulphurets in the rock show little sign of decomposition; and this is true even in layers of the propylite, that are fissured and seamy and drenched with water, whether hot or cold. In fact, the preservation of the sulphur compounds, in presence of so much heat and moisture, is a noticeable fact, which the author has frequently remarked in all the mines. The analyses of such of the mine waters as he has been able to find confirm this statement.

The author says the quantity of water pumped from the mines the past year must have been as much as 350,000 or 400,000 tons a month. If its temperature is assumed to be only 135° F., and the average temperature of the air for the year 50° F., we have in the year, say, $350{,}000 \times 12 = 4{,}200{,}000$ tons of water raised 85 degrees in temperature; or, as the usual expression is, $4{,}200{,}000 \times 85 = 357{,}000{,}000$ ton-heat units have been absorbed by the water. If the heating-power of anthracite coal is estimated at 7500 heat units to the ton, the heat in this water is as much as would be obtained from the combustion of 47,700 tons of coal. A cord of pine wood weighing 2700 pounds will probably give about 4300 heat units in practice, so that 84,000 cords would be necessary to keep up the heat withdrawn from the rocks in the mine waters alone.

If 10 tons of air pass through the mines collectively each minute, or 14,400 tons daily, and the air when discharged from the mines has an average temperature of 92° F., the total quantity of air for the year will be 5,256,000 tons, and the average rise in temperature 42 degrees. The specific heat of air being 0·267, we have—

$$5{,}256{,}000 \times 0{\cdot}267 \times 42 = 58{,}940{,}784 \text{ ton-heat units}$$

for the amount of heat absorbed by the air. This corresponds to an expenditure of 7859 tons of anthracite coal, or 13,707 cords of wood. The total quantity of heat carried out of the mines yearly by the water and air is therefore 416,000,000 ton-heat units, to produce which, in ordinary industrial operations, would require 55,560 tons of anthracite, or 97,700 cords of wood.

The number of men employed under ground in the mines of the upper Comstock is less than three thousand, and the

heat from their bodies, together with that produced by the burning of the large numbers of candles, could not account for any considerable proportion of this heat. Indeed it may be assumed, in the absence of calculations, that all the heat from these and other ordinary sources of heat in mines is no more than sufficient to compensate for the large amount of refrigeration produced by the liberation of the compressed air which is employed in every mine to work numerous underground machines. This heat absorption has not been taken into account in the above calculations.

In another respect, also, these calculations are defective, and give results very much too low. Usually the air enters the mine dry and leaves it saturated with moisture, the evaporation of which indicates an amount of heat absorption, which would probably increase the above figures surprisingly.

These calculations, imperfect as they are, show that the source of heat is one that acts on a magnificent scale, and also that it cannot reside in the small quantity of pyrite which is oxidised. That source is probably the chemical alteration of the felspathic minerals of the propylite and other rocks. This change consists apparently in the process of transforming felspar to clay, technically known as kaolinisation, from the fact that china clay, or kaolin, is produced in this way.

With regard to the future increase of the heat it is judged that, until water temperatures above 154° F. are observed, nothing in the present condition of things indicates the certainty that the heat will ever rise to the boiling-point of water, 212° F. It is rational to suppose that the access of atmospheric air and water must diminish in proportion to the depth after a certain point is reached. At that point the temperature will be at a maximum. Below it there will be a state of equilibrium, probably for a very considerable depth. Below that the heat may diminish even to a point below that of the highest of the three zones. There must be some point where the absence of drainage allows the water to act like a blanket over the rocks, protecting them from the action of air or gases from the surface. The known depth required for the production of a temperature amounting to 130° F. is so great that we may fairly doubt whether air or water penetrate to lower depths in quantity sufficient to maintain mineral decomposition with the activity necessary to obtain the boiling temperature.

The author's explanation of the heat phenomena connected with these remarkable mines supposes the existence of a

cold, and what may be called a burnt out, layer of rocks, extending for 1000 feet below the surface, a zone of hot rock still in active decomposition, which has been found to exist for a depth of about 1500 feet more, and no doubt extends thousands of feet further, and, finally, a mass of cold rock at a great depth, which has not yet begun to decompose.

The peculiar bands of hot and cold rocks which the author describes are simply layers of rock in which decomposition has been delayed or hastened. When the texture of a rock is such that it resists decomposition longer than other layers in its neighbourhood, it will be at its maximum temperature long after its fellows have passed theirs and cooled down, and this the author conceives to be the situation of the hot bands. They are individual layers of rock undergoing delayed decomposition.

On the other hand, when a rock is peculiarly susceptible to the action of the air and water, its alteration will proceed more actively than that of the surrounding rock. It will, therefore, pass its maximum temperature sooner, and be cooled down by the time that its neighbours begin to be at their hottest. This is the state of the cold bands. These bands, in fact, offer at several places in the mines examples in miniature of the action that is going on upon a grand scale throughout the whole system of rocks.

All the known facts strengthen the supposition which is advanced in this report, that the heat in the mines is subject to a steady and moderate increase as their depth is increased, this comparatively regular progression being broken by the passage through belts of rock heated above the average of the "country."

The author then considers the relation of temperature to depth, and concludes his paper by remarking that the Comstock mines offer a greater promise of discovery in this matter of rock temperature than any other he is acquainted with. The extraordinary rapidity with which their operations are prosecuted, the extent of the works, and the fact that they open to inspection a great eruptive mineral lode thoroughly for two miles in length, and partially for many thousand feet more, give them unusual value as a field for investigation. They not only follow an eruptive dyke throughout its course, but they also explore a parallel system of eruptive rocks by crosscuts, which are often from 300 to 500 feet long, and sometimes stretch out to 1000 feet and more.

They are also certain to be opened to much greater depths than now, and with a rapidity that will no doubt make them

foremost in deep mining within a few years. These conditions, combined with the peculiar susceptibility of the country rock to decomposition, give good reason for expecting that they will before long be the scene of thorough and perhaps conclusive studies in this interesting subject of earth temperatures. This was but a secondary part of the author's work, which was chiefly confined to a geological study of the lode.

IV. MATTER ACTIVE.

"Tandem venitur ad formam substantialem qua adhuc abstracta per intellectum, remanet quoddam valde occultum quod est prima materia."—ALBERTUS MAGNUS.

JOHN DALTON.—You spoke to me lately of matter being dead. Death implies previous life, and in matter you will allow also that it implies capacity of living. We cannot after all expect matter to be alive of itself,—*i. e.*, isolated from the universe,—and is it not true that physicists have a general belief that when heat is gone the worlds cease? Human beings may be taken to illustrate this. If we consider any act—our walking, for example—it is only in part an act of our own; we rise up, and immediately gravitation draws us down; and if we look at dancers this mutual action between the earth and man is more apparent. The dancer seems to be glad because, for part of a second, he has overcome the attraction of the great earth; but he need not exult long, since down he must come, and the rhythmic struggle is a graceful tune of motion. It is a combined action of man and external nature, and similarly the elements are inert only when there is no sympathetic power to act with them.

Roger Bacon.—The analogy requires more than sympathy to complete it. The human being has a force within himself, and do we not find that analogous preparation in the molecules? The attraction of the earth is only a mean part of the work in dancing; the sun, too, if it does warm the elements and allow them to act, does no more; it gives no vitality that we know of, even when it removes that stiffness

of cold which, as we before agreed, brought death to the matter which we see. Why, then, do people preach to us about the sun being the life of the earth and the source of all life? To those who say so I turn, with apparent contradiction, and say—No, the life is quite as much in the molecules. The sun gives probably several motions, but it cannot transfer its vitality to stones; the composite organism receives it, and disposes of it; the poor organism of the flint only becomes warm; the complex organism of the man glows with all his soul, the less complex molecules are moved in a still varied way. If they are of one original material, it may be asked, why does not heat set them in motion in exactly the same way? But we find them showing nearly seventy characters, *i.e.*, we have seventy bodies with fundamental differences to external appearance and tests. This is certainly not according to the development theory as we find it taken for granted in early writers, who uphold the existence of a *prima materia* or an undefinable *Yle*. Without heat it was agreed that there would be no action; with heat the elements develope many and peculiar characteristics. The first is simply that the motion which we call heat is transferred to them, an action of the same kind in all bodies whether solid or gaseous elements; by it iron and oxygen both have their particles more violently agitated. The next motion is different; it arises from a characteristic of the molecule—the oxygen and the iron unite. This shows a compound character in these substances; one for motion simple (let us suppose) in heat, another for combining. How many more we do not know, but certainly the second has very many modes of showing itself. If heat warms up the molecule its hidden qualities come out (just as we find when a starved man is set by the fire to warm), and all the various properties of the elements come forth more thoroughly than character from seventy people. In other words, the molecules have character; and is it not a general fact in Nature that character of any kind has some organisation to produce it? If we wish to give a compound mechanical nature to a bar of iron we make out of it several parts, and produce a more or less composite machine, and according to the work it does is the amount of organisation given to it. In animals there are found organs, material embodiments of character, and so in plants, although when we go down very low in size, or high in subtlety, we cannot see them. If a molecule can do several things, is it not probable that it is because of these several parts? Heat cannot make a simple piece of iron do the work of an engine; why, then,

should we suppose a new law for a molecule, making a simple body do complex work?

John Dalton.—Allowing this argument to have some force, I cannot look on it as a final proof, and, even if it were, it is not any proof whatever that the original substance from which these atoms, as I call them, may have been made is the same for all; we may make our wheels of wood, iron, or brass.

Roger Bacon.—True; but all the organisations of plants and animals are made of the same materials, and it would appear that the organisations of the universe are also made of the same substances as the earth. If we seek a more simple substance, we may be satisfied that the same will do for all. There seems an analogy between your atom and a mechanism, and for my part I look on all the molecules of the elements as little heat engines; they do nothing until they are driven. This seems at least to apply to their chemical qualities. Interrupt a fly-wheel, and we have friction and heat; interrupt a molecule by some other molecule with which it combines, and we have heat also, and a stoppage of its ordinary motion; work in this case is done, and this work is the fabrication of a new product, a facture by the hands of Nature—a manufacture.

John Dalton.—I could agree to this facture, as you call it, in a sense; but are you not simply going back a stage to meet the difficulty as great as ever? If you make the usually received atoms of Newton to combine, do you not really do enough, giving them at their origin characteristics now found impressed on them by Nature?

Roger Bacon.—I imagined that an answer had been given to this: if we do so we make a great many compound bodies as the act of the first creation, and I might as well say why not suppose all the compound rocks to be made at once? why not allow the fossils to be the result of "a striving of inorganic nature after organic forms"? why do not all men rise ready grown or made from the red earth? This does not seem to be the will of Nature in any of the departments we have observed; it is unfair to suppose it in the small masses we speak of. This is an argument of probability, the compound character of your atom is an argument of fact, to my mind.

John Dalton.—You have quite forgotten one point. The atoms are not dependent on heat; they have the power of making it. They all gravitate, whether hot or cold, and if they were only allowed space they would rush together and produce heat in abundance, to give scope for all their chemical activities.

Roger Bacon.—We spoke lately of the very ingenious speculation of making supposed matter from ether; it agrees with the opinions of—let us say, for example—Albertus Magnus, who said, as the heading of this article denotes, that having taken away all the accidental forms we arrive at a substantial form, which being removed by the intellect there remains a something very occult, which is the first matter.

John Dalton.—I know something of the search for the *prima materia*, the something more than the elements, the fifth essence. Do you mean me to return to that mode of thinking from which, so happily as I think, Europe is freed?

Roger Bacon.—If our present atoms are compounds you must return so far as to seek for a substance out of which they were made, and I think that you are called on to do so intellectually even now, although I do not see a clear way of experimenting definitely. You have alluded to gravitation as presenting a mode of producing heat and setting all the phenomena of chemistry in motion, and certainly that power is wonderful. But if we were right before, then gravitation comes in after the atoms—molecules—are made. If the atoms were made from some other substance, we have no idea what that is, and cannot at any rate suppose it to gravitate; if it did we should find it accumulated surely; or if it could not be found by our usual observation it would be, to say the least, different from all our matter as we find it in the elements. We are therefore driven to a something which we cannot perceive, and which is not known to gravitate. If it does not gravitate it could not make these concussions to which you allude, and which produce heat. This matter, ether or otherwise, cannot therefore have made the suns by such means, and as a consequence the suns would not be the original source of the elements. This logic is not absolute, because we can suppose that some other power has brought the matter together; that, however, takes us another step back.

John Dalton.—You mean to say that you do not know anything to make atoms of, and you rush to a supposed something. I have them already made out of nothing at once.

Roger Bacon.—The elements all differ among themselves, of course; but some are like others, and show a brotherhood distinctly, as if the children of the same parents. Then the atomic relations distinctly point to parts made up in some manner, as distinctly as any of the qualities of com-

pound bodies point to more than one part, as distinctly as potash and soda pointed to a decomposition in the eyes of Davy. We are therefore compelled to look for a something out of which they were made, and it is quite fair to suppose that body to be hydrogen, at least for the present, and that is a body with weight. My opinion, however, is that it is too like other elements to be the original one; it is too complex in character. I look to a simpler. I have called it *Yle*, or simply matter if you choose, matter abstractly with none of the known qualities of elements. These must be added in the school of creation. I should like a power to make the elements out of this Yle. You offer me the attraction of gravitation, but Yle does not gravitate. I have tried to get the sun to make them, and I confess it is a power so tremendous that it is a fair field for much speculation. There may be various stages of heat, and the atoms may become various at these stages, and there may be other powers besides heat, but so long as we cannot tell what will produce heat in ether without the finished atoms our speculation is imperfect.

John Dalton.—In any case you are driven back a step, and your difficulties are not diminished. You have yet found no power to make your little engines, your molecules, your atoms of the present. For my part I see nothing beyond, and am satisfied to begin my study of creation as late in its history as their facture.

Roger Bacon.—I know that I am driven back, and I recognise a time of creation when all was waste and void, when matter reflecting light did not exist, when atoms with chemical properties were not, when suns were not present to attract and planets for moving round were not formed, when dust itself did not show its presence in space, and life was of course far from appearing. I have arrived at this point at which so many have arrived before me, looking at the eternal ether, if not in the same way as Aristotle did, and looking at the primitive material as far more subtle than our grosser bodies, partly as the alchemists did, but by no means exactly so. I gave this idea in my works—that Yle produced all things, and that through it everything could be changed into everything. You will readily see that, if the elements are made of an antecedent, the step can be by the same power reversed, and matter may lose its existence and pass into ether. We can then suppose the sun to be a great agent for breaking the elements up into their primary parts, as well as we can suppose it making elements out of the *prima materia*. In whatever way we come to this *materia*,

whether by beginning with it or returning to it, we are obliged to think of power—By what means did it produce atoms? The ether as it exists, be it ether or gas, conveys power; it therefore holds it in possession for a time; heat and magnetic influence from the sun are proofs. Some men tell us that power exists only in connection with our elements; this is not known to us; we judge of its amount and quality by its action on our elements, but we cannot suppose it arriving from the sun without passing the intermediate space. Power, therefore, exists of a kind very quick and subtle, and of a kind different from heat, and that power or these powers may be able to transform the elements, whether that itself be convertible into heat or not. There may be many powers made out of one original power, and this would seem better proved than that there are many elements made out of one original element. In any case their wondrous effects are partly seen, and the existence of these agents give us some clue to the very numerous phenomena which Nature produces—all of them, one might say, quite inexplicable. He that says that he understands one of them, let him explain. The object of this is to show that heat by concussion is not the original phenomenon; that heat also is not the original in all probability, but that elements must be formed before either exist. Something out of which they were formed, or are being formed, must have existed, or probably does exist, and power at any rate we must have to do anything, and that power is as mysterious as ever. If this is right reasoning, the history of the Universe has other stages than have been conceived; and as the body of man decomposes and composes, producing his life, so the Universe may be breaking up its matter and forming new matter at the same time. By this idea another glimpse is given us into the mode of comprehending the existence of time. We dare not say anything of the past and dread inconceivable eternity.

John Dalton.—When you get into that vague and poetical region I am obliged to assent, meaning that I believe that all is wonderful and mysterious; but have we made anything out of the discussion except this, that matter can do nothing without power, and that the power is not in the matter, such as we have it in our elements. If you mean so, I most gladly agree.

Roger Bacon.—We have made out that the power began not by forming elements, but an original something very subtle, and I repeat this to fix it on your mind, because I feel sure that you will soon give up supposing our atoms to

be simple. This is an important conclusion, because it not only means that these little engines—atoms—cannot work without power, and they could not be made without power, so that, instead of being the actual powers that govern the world, they are in a double stage of helplessness, both requiring to be made and driven like other machines.

John Dalton.—You certainly gain a view farther back in creation, and have an idea of evolution of matter by this mode of thought, and no length of stages can change my knowledge that power must exist originally. Even those who tell us that heat, and therèfore life, is got by gravitation merely reason in a circle; for what is gravitation? I am quite ready to believe that it is one of those characteristics that have been given to the elements, from which you are fairly entitled to call them compound; for even in my system I give a very compound nature without having thought of a compound structure to account for it.

Roger Bacon.—We must not stop here. The farther we go back the less does it appear to us that our matter has great power; it seems neither to make nor to unmake itself, and those who look to matter as beginning with excess of power and gradually dying out, only renewing itself partially by concussions until the final destruction, make a strange supposition, because the supposition involves a beginning of power made by matter, and a beginning of eternity, if we may so speak, as well as an end, so far as active matter is concerned. I see neither and can comprehend neither, and it seems to me that we are led in this reasoning back to power quite inconceivable developing matter: why this power did it so once and does not do it again there seems no reason for thinking, and it is possible to suppose this creative power as eternal more easily than it is to suppose the mere work which it performs to be eternal. If it worked before our mountains and our earth, it may work after them. It is just possible that the sun may prove a focus for Yle, to which to rush and be made into elements, and thus accounting for gravitation to the sun; but this would not account for mutual action. It is also possible to think that the sun may be using old material, making it new, and thereby lighting up creation with the rubbish. But whatever be the case our matter is not active enough to keep the Universe in motion of itself, and at best it may retain an impetus which may keep it going forward for a time, but of itself it is a slave; it works only when it is driven; and we are driven to conclude that neither from matter of our elements nor ether is obtained the power or forces that rule them.

We have been obliged to go farther back than the formation of our elements to seek the power, and lest the clue of the reasoning be lost I shall repeat the stages:—

The elements known to us will not act without heat.

They are therefore without life in themselves.

Heat is conveyed to them; from the sun, for example.

There must be an agent of communication if heat be mere motion.

This agent we call ether,—let us spell it æther,—and to convey vibrations it must be material. (In excessive cold, communication of heat even from solid to solid can only be by means of æther.)

It would seem that æther does not gravitate; gravitation is therefore not an essential of matter.

If we freeze down our elements to the most inert condition known, they still gravitate.

Therefore they are not the simplest matter; they have a quality beyond that which æther has.

If made of anything we suppose the simplest thing we know of—we call that æther, but there may be a simpler still—let us say Yle.

Then as to priority. Heat made by the rushing together of the elements could not have been the original act of creation, because the elements, as we have them gravitating, require first to be made; for we have seen that æther, or matter, is free from one of the most general qualities of our elements. It is simpler.

Heat therefore is not the great fundamental power of Creation.

Heat is a power to drive the elements when made.

We come, then, to an original, or at least an early, matter, less like matter than our own, and we seek a power to make elements from it.

Heat does not give life or activity to the elements; it only produces one of the conditions in which they are able to act. Having various characters they must be variously organised, and heat sets them in motion accordingly.

Whenever we follow matter carefully we come to helplessness, immobility, death, unless revived from without. It is set in motion by a something which is manifestly separate from itself. But there are many intricacies in the thought, and we shall leave it for a while, if you please.

John Dalton.—There is nothing in your ideas opposed to mine. I was contented with that which I saw, but I must confess that you are more likely to be right than I was, in boldly making the atoms engines, as you call them, or com-

pounds. I called them simple, and yet in reality I gave them so many properties that they became each a focus of powers instead of mere atoms, and I am willing to look farther back as you do for the beginning of things.

V. SOME NEW OPTICAL ILLUSIONS.*

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IN the Transactions of various learned bodies—the Royal Society, the British Association, &c.—papers have appeared from time to time describing various Optical Illusions. Some of these illusions have depended upon the duration of retinal impressions, some upon the formation of accidental subjective images, some upon the dispersion or irradiation of the eye, and some upon the phenomena of binocular vision. The illusions to be described in the present paper do not fall exclusively under any one of the heads enumerated, though they depend upon the duration of visual impressions, and upon a further and less perfectly understood property of the retina. They are all dependent upon *motion*, either of the object or of the observer, or of both. In each case that will be here brought forward there is a movement of the object across the field of view, and consequently of the image across some portion of the retina.

The most frequent illusions which arise thus are those in which one form of motion apparently takes some other form. As a most familiar instance of this kind of illusion we may take the case of the apparent motion of trees, hedgerows, and houses, as seen from a rapidly-running railway-train, the deception of the senses being most complete when the personal sense of motion is least.

When the train in which you are seated is drawn up beside another train, and then moves slowly forward,

* The greater part of this article was read before the British Association, at Plymouth, in September, 1877, an abstract only having as yet been published. A few additional facts were recently communicated to the British Naturalists' Society, and are embodied herewith.—S. P. T.

smoothly and without jolting, it is extremely difficult to tell whether your own train or the other one is in motion.

So when light clouds are drifted across the moon, one can frequently hardly resist the notion that it is the moon that is sailing along amongst fixed clouds; and if the drifting of the clouds be due to an upper current, while the lower air is still, the impression that the moon is sailing along past the clouds asserts itself with remarkable force.

I have observed an illusion closely akin to this at Clifton. Underneath the famous Suspension Bridge a zigzag path winds up to the top of the cliff, shaded overhead by trees. Walking up this path you see the bridge at intervals between the boughs, and, as the body rises and falls with the motion of each step, the bridge appears to be swaying violently up and down, as if it were blown about in the wind.

Many illusions akin to these very simple phenomena have been recorded from time to time. Three times—in 1845, 1848, and 1861—the late Sir David Brewster drew the attention of the British Association to some phenomena seen in railway travelling. If from the window of the carriage you look out at the pebbles and stones lying beside the line, you catch merely vague stripes, due to the rapid motion of their images across the retina; but on suddenly shutting the eyes "a motion is perceived in a direction transverse to the real impressions on the retina; and there is the appearance of lines complementary in the same transverse direction."* This Sir David subsequently referred to a subjective opposite motion going on simultaneously, and so causing a compensation of the impressions moving on the retina. In 1861 he returned to the observation, and compared the phenomenon with that obtained by watching the motion of a rotating disk with radial markings, directing the eye first to a point near the circumference, and then afterwards to a point near the centre, where the motion was slower. He concluded that there was a neutral line across the retina at which the compensation of the subjective impression was complete.

In the "Philosophical Magazine" for 1834 (p. 373) R. Addams described a peculiar optical phenomenon. After looking for some time at a waterfall, and then at "the sombre water-worn rocks immediately contiguous," he "saw the rocky surface as if in motion upwards with an apparent velocity equal to that of the descending water." This he ascribed to an unconscious recurrent movement of the

* Brit. Assoc. Report, 1845.

muscles of the eye-ball, continuing after the gaze had been directed to the rocks, and thus occasioning a displacement of the images on the retina.*

This illusion becomes more remarkable in the slightly varying case now to be mentioned. Watch the water of a rapid river, such as the Rhine immediately above Schaffhausen. The middle stream is running forward very rapidly. After watching it fixedly for some time, transfer your gaze to the slower stream near either bank. It actually seems to be running back.

I have also noticed, after watching a procession, that stationary objects appeared for a moment to be moving in a contrary direction.

In the "Journal of the Royal Institution" (vol. i., p. 609) an anonymous writer records a curious observation, that from a slowly-moving railway-train, while the stones and sleepers beside the line appear to fly back past the train, the neighbouring set of rails seems to be flying forward and keeping pace with the train. This he refers, and doubtless rightly, to the fact that the rails are of nearly uniform tint, and destitute of markings that would produce upon the retina impressions like those of the adjacent objects.

The railway affords many other instances of optical deception, and of these I will mention a few of which I am not aware that any specific notice has hitherto been taken.

When a landscape is observed from a moving railway-train, all distant objects from the near hedgerows to the distant hills appear to be moving past in the opposite direction, the nearer objects having the greater apparent velocity. Consequently, if the attention be fixed upon any object at some distance from the line, all objects beyond will relatively appear to be moving forward with the train, while objects nearer appear to be moving backwards. The combined effect is to make the entire landscape appear to be *revolving centrally* round whatever point we fix our attention upon.

Falling rain seen from a moving train always seems to fall obliquely (except in a *very* strong gale in the direction of the train's motion) in a direction opposite to that of the motion of the train. But if another train happen to pass in an opposite direction, and we look out at this and follow it with our eyes, rain-drops falling between the two trains will seem to be flying forward with ourselves.

* An account of a very similar observation was communicated by Mr. J. Aitken to the Royal Society of Edinburgh, in November, 1878, apparently without any knowledge of the observations of Addams, Brewster, or of the author of this article.

If we stand on the platform of a station and watch a train approach, the end of the engine appears to enlarge or swell up as it approaches and occupies a larger area of the field of vision. Conversely the end of the last carriage of a retreating train appears to shrink down and contract as it diminishes in apparent magnitude. Stationary objects by the side of the line similarly appear to swell up as we approach them in a train, and to shrink together as we retreat from them. Curiously enough, this motion is also one which calls forth a certain "compensation" in the action of the retina. For, suppose we have been watching objects enlarging as we approached them, and then suddenly transfer our gaze to the side of the carriage opposite to us, we shall observe that it is apparently shrinking together and retreating from us. The opposite effect—that of apparent enlargement and approach—is produced as a subjective compensative action after watching objects from which we are retreating. The effect is more amusing if, after observing either of these cases of motion, we transfer our gaze to the face of a fellow-passenger sitting opposite.

An observer at some slight elevation above a railway, seeing two trains pass along the lines simultaneously in opposite directions, will receive the impression as of one long train moving round a circle. For when you look at a revolving wheel nearly edgewise, the nearer edge is seen moving past the farther edge, and in an opposite direction. The apparent motion of the two trains is the converse of this impression.

If from a similar situation two trains are observed, one moving rapidly, the other slowly in the same direction, the slower train may appear indeed to be moving in an opposite direction—a phenomenon similar to that of the Rhine above Schaffhausen already noticed.

Dr. F. Guthrie has noted the following illusion :—"Looking at the arms of a windmill in motion, in the twilight, or at such a distance that their attachment to the mill is obscure, we can, when the aspect is very oblique, easily imagine the arms to be turning in the opposite direction. We then fancy we are looking at the other side of the mill: so that if the sails are actually towards us in their descent, we fancy them away from us in their descent, which gives the notion of rotation in the opposite direction. This hallucination can, after a little practice, be as readily controlled by the will as can the introversion of a linear drawing representing a solid." *

* GUTHRIE, Magnetism and Electricity, p. 243.

An analogous illusion is produced by illuminating certain vacuum-tubes with the sparks of induced electricity discharged alternately in opposite directions,* when the tube appears to be rotating about an axis perpendicular to its length and to the line of vision.

A crow flying along at dusk, seen against the sky at a low altitude, shows, when passing the observer, his wing above and beneath his body alternately. The effect of this alternation is as if he had but one wing, which seems to revolve round like the blade of a screw-propeller about its axis.

I have frequently stood upon the lofty suspension-bridge over the Avon, at Clifton, when large ships have been passing beneath. Under these conditions a curious illusion may be observed. If you look perpendicularly down on to a ship, as it emerges from beneath, it appears to be heeling forwards on to its bows; for as the masts emerge from under the bridge, and you see them growing longer as the foreshortening effect passes off, the mind cannot resist the notion that—like the windmill-sails—they are revolving round a centre. I have pointed out this effect to several persons, who have expressed much surprise at the completeness of the illusion.

The last set of illusions which will be described took their origin in an observation made by the writer early in 1876. He had been drawing a series of concentric circles in black and white, for the purpose of testing the astigmatic condition of the eye. Happening to shake the paper upon which the diagram was drawn, he observed a peculiar motion of apparent rotation of the circles. This illusion is extremely curious, and very easily reproduced. Let concentric circles in black and white be described upon a piece of card. If this be held firmly between the thumb and finger of the hand, and then a slight but rapid circular shaking motion be imparted by the wrist and elbow, the circles will appear to rotate upon the card. The hallucination succeeds best if the circles be clear and sharp at their edges, and the successive rings of black and white of equal widths. Their number and width is immaterial, but there seems to be a particular distance from the eye for each width of successive rings, at which the illusion succeeds best. Finely-drawn narrow rings must be held near, to produce a maximum effect; while to enable a number of persons to see the illusion at once the rings may be half or three-quarters of an inch in width, and to the number of fifteen or twenty.

* See S. P. Thompson in Phil. Mag., 1876.

The radius of the circle of imparted motion should equal the width of a black or of a white ring, and the rapidity found most successful is that when each rotation occupies from one-sixth to one-fourth of a second. The rings appear to rotate once for every complete motion of the hand and card in the circular path, and in the same direction as the imparted motion. In this experiment each ring is displaced to a distance equal to its own breadth in every direction successively around its centre; and as the impression remains a short time on the retina, the optical effect is equivalent to that of a ring eccentric to an equal amount and actually rotating. Hence the illusion.

I have constructed a large number of patterns of curvilinear, circular, elliptical, eccentric, and concentric lines, many of which exhibit, in whole or in part, the same phenomena of apparent rotation. One of these is a single black circle, having a number of internal cog-teeth, upon a white ground. This circle, when shaken circularly in the manner described, appears to move round in the opposite direction to the imparted motion, and to move round through a distance of but one tooth for each successive complete motion. For circles possessing this property I have suggested the name of "Strobic Circles." Their motions are best seen when the eye is directed not exactly at the circles, but at some point near them. I have therefore found it more effective to have two strobic circles drawn side by side upon one card. That circle rotates most obviously on which the gaze is *not* fixed.

Further, I have noticed that if a strobic circle be "rotated," while a number of other circles are lying stationary within the field of view, when the eye was directed to the moving circle the others also began to "rotate."

This last observation cannot, I think, be explained on any supposition of unconscious muscular movement. In fact I entirely doubt the validity of this hypothesis in the case of Addams's observation upon the waterfall before cited.

I am inclined rather to attribute these effects, and those of "compensation" in general, to waves of nervous disturbance moving over the retina; these waves, being of two orders,—one primary, and in the same direction as the objective motion of the images upon the retina; the other secondary and later in time,—giving rise to the subjective motions of compensation. I do not see how on any other supposition the phenomena noted in an earlier paragraph relative to compensative shrinking or expanding of objects can be explained. Such a hypothesis, will, I believe, also embrace all the other phenomena of apparent motion, except those which

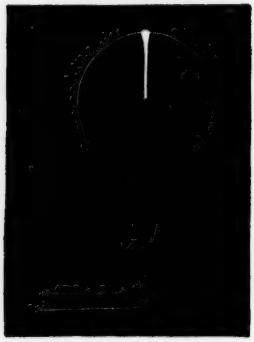
are the result of mental associations alone—such illusions, in fact, as those of the windmill and the flying crow. Such waves of nervous disturbance have, it would seem, a definite rate of propagation, probably not independent of the nature of the moving image with respect to colour, relative luminosity, and apparent magnitude. But whether these waves of sensible impression are due to a physical motion of any structures of the retina I am not yet prepared to offer an opinion.

VI. ON ELECTRICAL INSULATION IN HIGH VACUA.*

By William Crookes, F.R.S.

THE following experiments were suggested in the course of an investigation on the passage of an induction current through highly exhausted tubes. The main branch of the research being likely to occupy my attention

Fig. 1.



for some time, I may be unable to return to these less important offshoots.

A pair of gold leaves were mounted, as for an electroscope, in a bulb blown from English lead glass tubing. The leaves

* A Paper read before the Royal Society, February 20th, 1879.

were attached to a glass stem, and the lower part of the bulb was drawn out for sealing to a Sprengel pump, as shown at Fig. 1. A stick of ebonite excited by friction was generally used as the source of electricity, but any other source will do equally well, provided it is not too powerful.

No special attention was paid to the action of electricity on the leaves in air or at moderate vacua, as it agreed with what is already well known. The exhaustion was pushed to a very high degree (about the millionth of an atmosphere), when it was found that the excited ebonite had a much greater effect on the gold leaves than at a lower exhaustion; for a long time, however, I was not able to charge the leaves permanently, in consequence of their falling together as soon as the source of electricity was removed.

When a hot substance was brought near the bulb facing a gold leaf, so as to warm the glass, molecular repulsion took place, and the leaves retreated from the warm spot, standing out at an angle of about 45°. As the glass cooled the leaves resumed their former vertical position.

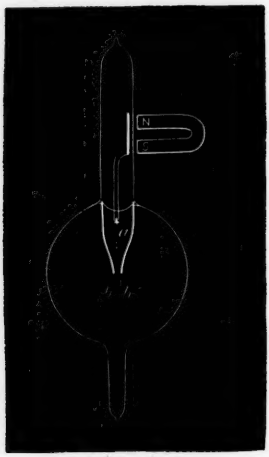
While the leaves were repelled from the hot glass the excited ebonite had a very powerful action on them, and if it were brought near hastily the leaves flew off to the side of the glass, destroying the apparatus. By careful management and repeated trials, however, the ebonite could be brought near the warm spot of glass, the leaves suddenly extending at an angle to each other. The appearance was as if a spark had been able to pass across the bridge formed by the line of advancing and retreating molecules connecting the hot glass with the gold leaves. On the ebonite being removed and the glass allowed to cool, it was found that the repulsion of the leaves was permanent. The rubbed ebonite would attract and repel them as it was moved to and fro, but the angle formed by the leaves with one another remained unchanged. A warm body brought near the glass opposite one leaf would repel the pair as a whole; on then warming the opposite side of the glass repulsion on that side took place, the angle of the leaves being somewhat diminished, but on cooling the leaves opened again to their former extent.

When the glass bulb was strongly heated by a spirit flame the leaves suddenly discharged and fell together.

Another bulb (Fig. 2) was prepared, containing a plate of mica, *a*, which could be suddenly placed between the gold leaves, *b b*. The plate of mica was longer and wider than the gold leaves, and was connected with a small piece of iron wire, capable of moving up and down a tube sealed into the

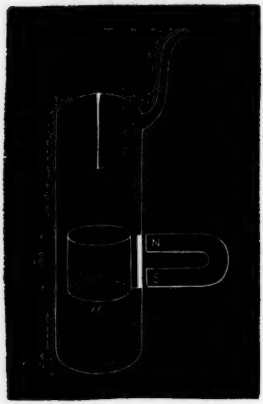
top of the bulb. By means of an outside magnet the mica plate could thus be lowered between the gold leaves or raised

FIG. 2.



out of their way, as desired. The tube was exhausted to about the millionth of an atmosphere, the mica plate being

FIG. 3.



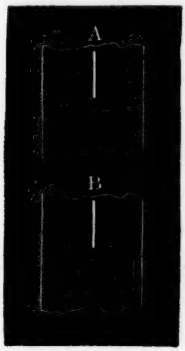
held quite above the leaves. One side of the bulb was then heated, and the leaves permanently charged by means of the

excited ebonite. The mica plate was now carefully lowered. As it came between the gold leaves they diverged farther apart, and kept so as long as the mica plate was between them. On removing the plate the leaves re-assumed their former divergence. This could be repeated any number of times.

A similar piece of apparatus (Fig. 3) was made, only instead of a mica plate coming between the leaves, a mica cylinder, *a*, capable of being raised and lowered outside the divergent leaves, was employed. I was not able to get entirely concordant results with this, owing to the friction of the mica developing electricity on the inner surface of the glass tube; but in all cases, when the cylinder was raised until it covered the electrified leaves, it had the effect of diminishing the angle which they formed with each other.

The following experiments were also tried:—The leaves being separated about 160°, as at Fig. 4, A, one side of the tube was slightly heated by a spirit-flame. The leaf on that side fell to a vertical position, and remained so when all was

FIG. 4.



cold, the other leaf sticking out as before, as at B. This would seem to show that the divergence of the leaves in this case was not so much due to their mutual repulsion as to an attraction exerted on each of them by the inner surface of the glass tube. The remaining divergent leaf could be slightly lowered when the glass tube above it was warmed with a bunch of cotton-wool dipped in hot water. On cooling the leaf rose again to its original position. When this side of the tube was also heated with a lamp, the leaf was repelled down, but not so readily as the other had been, and

when the tube got cold it rose to nearly its former position. This was repeated several times with uniform results. When the leaf was repelled down the vertical leaf also moved away, so as to keep the same angle between them. It is therefore evident that the leaves themselves were also charged.

FIG. 4.

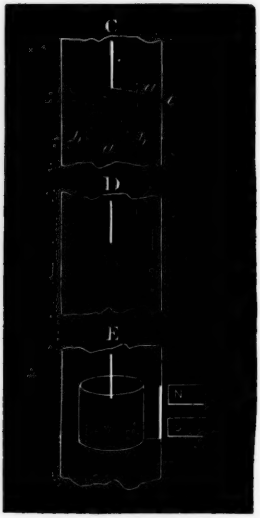


Fig. 4, C, shows the two positions of the leaves, *a a* before applying heat to the side *c* of the tube, and *b b* after heating the glass at *c*.

The tube was now heated on both sides, causing the leaves to come nearer together, as shown at Fig. 4, D. While the glass was warm the cylinder was raised so that it surrounded the leaves: this caused them to get a little closer together, and they kept in this position, shown at E, after the whole apparatus was quite cold.

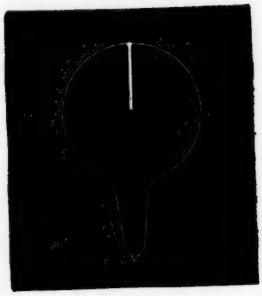
After remaining thus for some time the cylinder was lowered, and the leaves widened out and took up the position shown at *b b*, Fig. 4, C. They did not return to the position *a a*, showing that their divergence was now owing to their own mutual repulsion, and not to an attraction of one or other to the electrified glass.

In December, 1877, I totally immersed one of these exhausted glass bulbs in a vessel of water, the gold leaves

having previously been charged, and standing at an angle of 112° from one another, as at Fig. 5. The water was connected electrically with "earth," and the whole was set aside in a cabinet on the 1st of January, 1878.

At the present time, after having remained in this condition for thirteen months, the leaves form exactly the same angle with one another which they did when they were first put in the cabinet.

Fig. 5.



From this experience I think we may consider that at an exhaustion of a millionth of an atmosphere, air is an absolute non-conductor of statical electricity. It is therefore legitimate to conclude that the vacuum of interstellar space offers equal obstruction to the discharge of electrified bodies, without necessarily interfering with their mutual repulsion if similarly electrified. It is possible that in these facts an explanation may be found of some obscure celestial phenomena.

VII. SPIDER'S WEB FOR MICROMETERS.

THE web used is that of the common garden spider; the spinner of those geometric webs is abundant in summer and autumn. The reason why this spider is selected, rather than the house spider, may easily be seen by anybody who watches the proceedings of each when a fly is caught. The house spider seizes his victim and spins around him a winding-sheet of web, before carrying him

into the larder. The garden spider binds around his captive a cord of web. It is this cord that is used for collimation.

In the autumn the mathematical instrument maker goes on his spider-hunting expeditions, generally on Sundays. He carries some pill-boxes in his pocket; selects well-fed, full-grown specimens, and puts each in a separate box, knowing the savage habits of his six-legged friends; for if two or more were put together in the same box, only a collection of amputated limbs and mangled bodies would be found on returning home,

The webs are secured for use and storage by making a fork of iron wire, 4 or 5 inches long, and 1½ to 2 inches between the bifurcations. The spider is held in the left hand and allowed to drop, which he readily does when dissatisfied with his quarters, but before falling he glues an end of cord to the finger, and then lets himself down easily by gradually spinning it out and hanging by it as it lengthens.

The instrument maker catches this cord across his fork, and, by turning, attaches it to one side; then he goes on turning the fork and advancing it, so that as the spider continues paying out his cable a series of obliquely crossing threads are wound upon the fork, which, when charged, is carefully laid in a box or drawer for use. The elasticity of the iron wire keeps the webs sufficiently stretched, and they are applied to the stop by simply laying the fork over it in such wise that one of the stretched webs shall fall upon the mark made on its face. When thus in position, a drop of varnish or glue, made by dissolving shellac in alcohol, is let fall upon each side; the spirit rapidly evaporates, and the web is fixed.

One of the odd results of this use of spiders is that many workmen become spider fanciers, and keep choice domesticated specimens that learn to spin their webs in convenient places above the work-benches, or in the bedrooms of their masters, who lovingly supply them with the fattest of bluebottles. Though I forsook the trade on the expiration of my apprenticeship, I have not yet lost my affection for these animals—I never wilfully kill a spider. A sad story is told of the desolation of the late Mr. Troughton, who on one occasion engaged a new housemaid, and allowed her to commence professional operations without receiving the usual injunctions concerning his pets.—W. MATTIEU WILLIAMS, *in Journal of the Society of Arts.*

VIII. THE "JUMPERS," OR "JUMPING FRENCHMEN."

DURING the past year I have been investigating a manifestation of the Involuntary Life of the most novel and interesting character. It is found among the French Canadians, and is there known under the expression "Jumpers," or "Jumping Frenchmen." It appears, according to my researches, that a certain proportion of that people—mingled French and Indian blood—have acquired the permanent habit, which they cannot control, of jumping, or striking out with their hands, when commanded to do so, suddenly and authoritatively, by anyone who chances to be near them. The habit appears to have been acquired, in the first instance, by tickling one another, in the winter camps where they cut lumber in the Maine woods. They are a somewhat degraded race of beings; have few resources, very little intellect, and no mental discipline; cannot, usually, read or write, and, in their camps, while away the long winter evenings by playing upon each other's ticklishness until some of them get into a state of abnormal susceptibility that compels them to obey, automatically and instantly, any sudden order, as to strike, or to catch, or to jump, or even to vomit; they are at the mercy of their companions, and are frequently so much annoyed that they have to leave the camp where they are employed. The more they are played upon the worse they become; for the habit grows with exercise. This condition is not an epidemic, but a fixed and permanent state, and, so far forth, is different from the phenomena so often witnessed in revivals. It is, in fact, as I have elsewhere stated, a liability to be entranced on slight excitation; differing from the allied trance to which all of us are liable only in this, that it follows a very much milder irritation. When these "Jumpers" are excited to jump or strike, or to perform any of their peculiar automatic acts, they present the appearance of entranced individuals; their faces turn pale, their eyes are fixed and glassy, and sometimes their limbs tremble. One of these Jumpers is a waiter, and when told suddenly to "drop it," he at once drops whatever he may have in his hand, though it may be on the head of one of the guests, or on the floor. Another has so susceptible a stomach that he at once throws up his

meals when any one but "gags" or makes the motions of vomiting in his presence; thus he has grown thin, and at one time was almost starved. One Jumper, when told to "strike," struck against a red-hot stove and burned himself. Accidents of this kind are quite frequent in their camps. One man, standing on the shore of a pond with a five-dollar gold piece in his hand, was told to "throw it;" he threw the money—a large sum for him—into the water. Another was standing near a kettle of fish; he was told to "jump," and he jumped into the kettle. When one of these Jumpers is addressed sharply and quickly in any language with which he is not familiar, he, at once and automatically, responds in that language. Thus, in numberless ways, they are abnormally susceptible to stimuli which, in the same degree, would have little or no effect on others.

In its relation to the subject of inebriety these extraordinary phenomena are of interest as illustrating the power and extent of the involuntary life, showing how varied and complex and subtle are the manifestations of this side of human physiology. These Jumpers, in the acts here referred to, are absolute automatons, utterly without volition or responsibility. Whatever responsibility there may be in these cases belongs to the time when the habit began to be formed—their first playing and trifling with themselves and others in the loneliness of their winter camp-life; they are no more to be blamed for their acts than are patients afflicted with St. Vitus's dance, or hysteria, or epilepsy, or with any form of insanity. The treatment, if any is used, should consist in removing the victims from the temptations of camp-life; they should be isolated, or, at least, kept away from those who are similarly afflicted, or who would take pleasure in playing upon, and thereby increasing, their weakness; at the same time, everything that educates and developes their higher cerebral centres will be of service. Indeed it has already been noticed that they grow worse by aggregation, and better by isolation. Their habit is a real and serious affliction to these people,—a source of anxiety and positive torment; they would rejoice to be delivered from it.

On the other side of the world, among the Malays, in the Island of Java, according to the "London Medical Record," phenomena precisely similar to those exhibited by the "Jumping Frenchmen" are seen. A woman carrying a child, and seeing one even pretend to drop any article, may at once drop that child. Many other interesting illustrations are given.

Very recently, also, my attention has been directed to some allied phenomena connected with a religious revival now in progress in a certain town in Vermont. The victims of this excitement roll on the floor or ground in most absurd and undignified attitudes; whence they are called "Holy Rollers." Unlike the Jumpers, however, these Rollers are not in a permanent liability to their disorder; when the excitement is over they will spontaneously recover. Just at present, in the height of the public enthusiasm, they are, on this subject, pure automatons. In the same line are the cases of starving girls, hysteria, and hysterical trance, like that of Mollie Fancher, of Brooklyn, which is now exciting so much enthusiasm. I have studied a number of similar and allied cases, and I never share the popular prejudice against them. They are without volition, practically irresponsible, and to be blamed—if blamed at all—for the *beginnings*, not the endings of their disease. Like inebriates, they are to be treated by taking them from their home and friends, and giving them a radically new environment.—G. M. BEARD, M.D., *of New York.*

NOTICES OF BOOKS.

Practical Physics ; Molecular Physics and Sound. By FREDERICK GUTHRIE, F.R.S., Professor of Physics in the Royal School of Mines. (London Science Class-Books Series.) London : Longmans and Co. 1878.

FIVE-AND-TWENTY years ago physical laboratories were almost unknown in this country. At the present time all our large educational institutions possess a laboratory of this nature side by side with one devoted to chemistry. The fine physical laboratories at Oxford, Cambridge, Glasgow, and South Kensington may be taken as examples of what such places should be like, and of how they should be worked. At the latter some dozens of students are successfully put through a course of practical physics every year, under the guidance of Prof. Guthrie, who has given us some of the results of his experience in the handy little volume before us. An admirable manipulator himself, he knows well how to instruct others; and this work will not only be welcomed by the student, but also by the older man of science engaged in research, who will find many useful hints as to the preparation and use of apparatus.

Starting with some definitions and experiments relating to the cohesion of solids, the author passes on to the cohesion of liquids and gases, introducing us to some of his beautiful experiments on bubbles and drops. We object, however, entirely to the term "cohesion of gases," and we cannot at all understand the scope or object of the single paragraph (No. 19, p. 18) devoted to the subject, which ends with the sentence—"A more exact method is to place timed chronometers under bell jars containing various gases, and also *in vacuo*." The term "volume-elasticity," in the next Section, is awkward; the piezometer, on the same page, could only be constructed and worked by a very skilful manipulator, and when in action would scarcely repay the infinite labour expended upon it.

The subjects of Effusion, Diffusion, and Occlusion are ably discussed, and the most recent researches in each case are introduced. The short but comprehensive chapter on Specific Gravities abounds with practical hints, and details all the necessary processes. The ingenious way in which Prof. Guthrie has applied singing flames to the demonstration of the refraction of sound by gaseous lenses (pp. 81, 82) is deserving of notice; also the graphic representation (p. 102) of the cause of the clap, rattle, roll, and boom of thunder. The subject of Interference is ably and clearly treated.

The Appendix is not the least valuable part of the book: it contains a number of useful hints as to glass-working, fusing platinum wires into glass, and so on. Finally, a list of the apparatus and materials necessary for experiments in sound and waves concludes a volume which we cordially recommend to the notice of all students of physical science.

A Dictionary of Chemistry and the Allied Branches of other Sciences. By HENRY WATTS, F.R.S. Assisted by Eminent Contributors. Third Supplement, Part I. London: Longmans and Co. 1879.

THIS very valuable and standard work, which is without doubt the most exhaustive Dictionary of Chemistry in any language, furnishes, by the aid of its Supplements, a complete record of chemical research, within a short time of its appearance in the journals of scientific academies. Research is so abundantly on the increase that this closely-printed volume of 838 pages forms only the first half of the Third Supplement. The second half will appear before the end of this year, and will bring down the record of chemical discovery to the end of 1877, including, however, the more important discoveries of 1878.

Among the noticeable articles in Part I. we may mention Dr. Mills's account of Cumulative Resolution, Dr. Thorpe on Flame, and Mr. Warington on Barley and on Forest Trees. But the great feature of the volume is, without doubt, the very exhaustive article—presumably by the Editor—on the Benzenes and Benzoic Acids, extending over more than 150 pages. This includes an account of Körner's elaborate researches on the Orientation of the Benzene Derivatives, published in the "Gazzetta Chemica Italiana," and now for the first time made known to English readers. To this subject we venture specially to direct the attention of chemists, as one which is well worthy of their complete consideration.

The subject of "Cumulative Resolution" (a term proposed by Dr. Mills) has been developed by Wurtz, Watts, and Mills. The action is defined as "the combination of a substance, or mixture of substances, with itself n times, a particular portion of it being lost each time, according to some fixed law. Thus, bismuthic nitrate, when decomposed by a gradually increasing quantity of water, yields a series of bodies, which are less and less nitrogenous, and more and more bismuthic. Having regard to the denitration alone, we write—

$$n(Bi_2O_3.3N_2O_5)-(n-1)N_2O_5=Bi_{2n}O_{3n},N_{4n+2}O_{10n+5},$$

and by giving various values to n, from 0 to ∞, we shall obtain the formulæ of all possible compounds between $Bi_2O_3.3N_2O_5$ and

$Bi_2O_3.N_4O_{10}$." It is afterwards applied to various minerals derivable from two silicic hydrates, of which the second is the cumulate of the first.

The present volume goes down to the end of F: it embraces all new matters of any importance in Inorganic and Organic Chemistry, and in Mineralogy. We do not notice any articles on physical or technical subjects, and the great mass of matter relates to Organic Chemistry.

An Elementary Text-Book of Petrology. By FRANK RUTLEY, F.G.S. London: Longmans and Co.

THE study and recognition of rocks may fairly be considered as the alphabet of geology. without a knowledge of which the student may certainly become a reader, but never a worker in the science. In other words, he may peruse handbooks and manuals, may fix their contents in his mind, and pass an examination with credit, and yet be utterly unable to make an original trustworthy observation or to verify the theories which may be put forward. It is therefore strange that certain geological text-books—to particularise would be invidious—overlook this part of the subject altogether. The reader is supposed to be born with the power of recognising the respective species of rocks, and therefore at the end finds himself no more of a geologist than he was at the beginning. Mr. Rutley, in the text-book before us, makes a useful and praiseworthy attempt to supply the knowledge thus found wanting. Into chemical methods for the diagnosis of rocks and their constituent minerals he does not enter at length, the rather as all needful information on this head may be found in well-known and accessible works. He occupies himself the rather with physical, and especially optical, characteristics. The microscope—thanks to the exertions of the late David Forbes, of H. Witham, and especially of Mr. H. C. Sorby—is now no less essential to the geologist than to the student of animal and vegetable life, and our author explains clearly and fully the applications of this instrument in petrological research.

We can strongly recommend this work to all who are entering upon the study of geology, and who wish at the outset to lay a firm foundation.

The Realistic Assumptions of Modern Science Examined. By T. M. Herbert. London: Macmillan and Co.

We were under the impression that men of Science fully, though tacitly, recognise the necessity of a general postulate underlying all their researches and speculations; to wit, that phenomena are what they seem, and act as they seem to act. No one, to our best belief and knowledge, ever supposes himself capable of furnishing an absolute logical demonstration that the impressions of our senses are an invariably accurate copy of anything external. What everyone thus fully concedes Mr. Herbert sets himself to prove. In reply to the man of Science who taunts metaphysicians with the vanity and baselessness of their endeavours, he brings a counter-charge, which is merely a learned and elaborate version of the vulgar retort "You're another!" But that he adds any degree of certainty to human knowledge, or shows how we may escape from the difficulties he has so acutely pointed out, we are unable to perceive, and consequently we can only regret that he did not employ his great powers of mind in some other and more fruitful direction.

CORRESPONDENCE.

THE CHARACTER OF THE SEXES.

To the Editor of the Monthly Journal of Science.

SIR,—Mr. Murphy, in his "Habit and Intelligence," remarks that in man only do we find a reversal of the usual character of the sexes. "In all other species beauty is developed in the sex where the passions are strongest, and consequently generally in the male." Other writers, in discussing the origin of man, seem to take a somewhat similar view. But if we could ask the lower animals, would not each declare that its own species was the single exception to the rule that the male sex is the more beautiful? Civilised travellers, while fully recognising the beauty of women of the higher race, not unfrequently pronounce that among the lower savages the men are passable, but the women utterly repulsive.—I am, &c.,

S.

TRANSFORMATION OF SPECIES.

To the Editor of the Monthly Journal of Science.

SIR,—I see it mentioned among the "Biological Notes," in your last month's issue, that M. Bordier considers atmospheric pressure as one of the principal agents in the transformation of species. If variation is most rapid where pressure is most intense, should not the deep-sea fauna be richer and more varied than it has been found to be during the late *Challenger* Expedition? Are there any observations to show whether variation is greater or less at the sea-level than on table-lands and mountain-slopes?—I am, &c.,

J. W. S.

PROCEEDINGS OF SOCIETIES.

ROYAL SOCIETY, *January* 23.—A paper by C. William Siemens, D.C.L., F.R.S., "On certain means of Measuring and Regulating Electric Currents," was read. It is well known that when an electric current passes through a conductor, heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time. Dr. Siemens proposes to take advantage of this well-established law of electrodynamics, in order to limit and determine the amount of current passing through a circuit. The most essential part of the instrument employed for this purpose is a strip of copper, iron, or other metal, rolled extremely thin, through which the current to be regulated has to pass. One end of this thin strip of metal is attached to a screw, by which its tension can be regulated; it then passes upwards over an elevated insulated pulley, and down again to the end of a short lever, working on an axis, armed with a counterweight and with a lever, whose angular position will be materially affected by any small elongation of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal, supported by means of metallic springs, so regulated by movable weights as to insure the equidistant position of each prism from its neighbour. The current passing through the thin strip of metal passes through the lever, and the line of prisms to the terminal, without encountering any sensible resistance. A second and more circuitous route is, however, provided between the lever and the terminal, consisting of a series of comparatively thin coils of wire of German silver or other resisting metal, connecting the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever and terminal respectively. Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 millim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost

instantaneously. But with the increase of temperature the position of the regulating lever is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown into circuit: the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate. The resistance coils, by which adjoining contact springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere. In dealing with feeble currents, Dr. Siemens uses another form of regulator, in which disks of carbon are substituted for the wire rheostat. The electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of say 0·3 millim. diameter is drawn tight between the end of a bell-crank lever, and an adjusting screw, the pressure of the lever being resisted by a pile of carbon disks placed in a vertical glass tube. The current passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximum; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance. The instrument first described may be adapted also for the measurement of powerful electric currents. The variable rheostat is in this case dispensed with, and the lever carries at its end a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil draws a second or datum line upon the strip, so adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant. The thin sensitive conductor may be utilised either to restrict the amount of electricity flowing through a branch circuit, within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

January 30.—"A Comparison of the Variations of the Diurnal Range of Magnetic Declination as recorded at the

Observatories of Kew and Trevandrum." By Balfour Stewart, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and Morisabro Hiraoka. In a previous paper by one of the authors a table is given exhibiting monthly means of the Kew diurnal declination-range, corresponding to forty-eight points in each year, or four for each month, that is to say, approximately one every week; and, in another paper, another similar table exhibits monthly means of the Trevandrum diurnal declination-range for weekly points. In the present paper these two tables are compared together. A comparison of the curves appears to lead to the following conclusions :—

(1.) Generally speaking, maximum points or risings in the one curve must be associated with maximum points or risings in the other, rather than with minimum points or depressions. Indeed, the researches of Broun and others, from a different point of view, strengthen this conclusion, which is, moreover, abundantly supported by a glance at the curves themselves ;

(2.) The oscillations of the Trevandrum curve are greater than those of the Kew curve ;

(3.) In many cases where there is a want of striking likeness between the oscillations of the two curves, there are yet noticeable traces in the one curve corresponding to the oscillations of the other. There are, however, a few cases where there is a want of apparent likeness.

(4.) In general, though not invariably, the oscillations of the Trevandrum curve follow rather than precede the corresponding oscillations of the Kew curve.

"On the Determination of the Rate of Vibration of Tuning Forks," by Herbert McLeod, F.C.S., and George Sydenham Clarke, Lieut. R.E. The paper commences with a description of the time-measurer adopted. The tuning-fork apparatus consists of a brass drum resting on friction wheels, and driven by a weight and train. Uniformity of motion being of great importance, an air-regulator, consisting of a fan enclosed in the lower compartment of a cylindrical box, is employed. Round one end of the drum are wrapped strips of paper on which white equidistant lines have been so ruled that they are parallel to the axis of the drum when the strips are in position. The strip most frequently used has 486 lines round the complete circumference of the drum. Opposite this graduated strip is placed a microscope with its axis horizontal. In the sub-stage is placed a 2″ objective, producing an image of the graduations at the focus of the object-glass of the instrument. At the common focus of the two lenses is placed the tuning-fork, the stem of which is held vertical in a vice. If when the fork is in vibration the drum is made to rotate with such a velocity that one of the graduations passes over the interval between two adjacent graduations in the time of one vibration of the fork, a stationary

wave is seen of length equal to the length of that interval. To determine the number of vibrations of the fork in a given time, it is only necessary, therefore, to be able to count the number of graduations which pass in that period. An electric counter gives the number of complete revolutions accomplished by the drum in any given period; and a fine-pointed tube, containing magenta, is carried by a saddle above the drum, and, being actuated by an electro-magnet, makes a dot on a piece of white paper wrapped round the drum at the beginning and end of the experiment. The distance apart of these dots gives the additional fraction of a revolution accomplished by the drum during the period of the experiment.

In illustration of the method a determination of one of Kœnig's forks was given :—

Duration of experiment, 5 minutes.
No. of 1st mark, 163. Number of line on circle, 301·0.
„ 2nd „ 164. „ „ „ 199·5.
Temperature by thermometer, 16·8°.
Number of revolutions shown by counter, 158.
Number of lines on circle, 486.

During the experiment the wave had risen in the field of the microscope one-half a wave-length above the hair.

$158 \times 486 = 76{,}788$ lines passed in 158 revolutions.
$301 - 199{\cdot}5 = 101{\cdot}5$ lines between two marks on drum.
$101{\cdot}5 - 0{\cdot}5 = 101$ lines corrected for movement of wave.
$76{,}788 + 101 = 76{,}889$ vibrations of fork in 5 minutes.
$\frac{76{,}889}{300} = 256{\cdot}2966$ vibrations per second.

When this determination was made four other measurements were carried out, lasting for different intervals of time.

5 min.	256·297 at	16·8° C.
2 „	256·298 „	16·9
3 „	256·293 „	17·0
1 „	256·292 „	17·0
4 „	256·292 „	17·2
Means	256·294	16·98
Correction for error of therm.		− 1·4
		15·58

Clock was losing about 0·8 second per day. Correction for clock rate—

$$256{\cdot}294 \times \frac{86{,}399{\cdot}2}{86{,}400} = 256{\cdot}292.$$

The maximum difference between the above numbers is 0·006, or 0·00234 per cent.

As frequent bowing might affect the rate, two sets of measurements were made, in one of which the fork was bowed every 20 seconds, and in the other every 5 seconds.

Duration of Experiment.	Bowed every 20 seconds.	Bowed every 5 seconds.
5 min....	512·470	512·460
3 „	512·460	512·452
3 „	512·460	512·456
Means ...	512·463	512·456

No change of phase has ever been observed on the application of the bow. The same result was obtained by Lissajous, although Poske has since found that bowing produces an alteration. The authors have never seen any sudden jump of the wave during bowing. To test this in another manner two forks were arranged, one horizontally and the other vertically, so that both could be seen simultaneously in the microscope (the horizontal one being beyond the sub-stage). The Lissajous figure is seen at the angle made by the two forks. When bowed no change of the form of the figure took place; its amplitude merely was increased. This arrangement of the forks is suggested as a method for comparing two forks of nearly the same pitch, or of a fork with its octave. A difference of amplitude produced no appreciable change in the rate of the fork.

	Large Amplitude. 0·37 m.m.	Small Amplitude. 0·15 m.m.
	256·277	256·275
	256·273	256·274
Means ...	256·275	256·2745

The mode of fixing the fork in a vice might have changed the rate: to test this two sets of measurements were made, in one of which the fork was fixed in the vice, and in the other on a sounding-box.

	On Sounding-Box.
	512·489
	512·482
	512·477
Mean ...	512·483

This result may be compared with that above given in which the fork was bowed every 20 seconds. It will be seen that the rate is rather less when the fork is held in the vice. In the previous paper the coefficient of change of rate for temperature is given as 0·00011 for each degree centigrade: this number has been fully confirmed by numerous measurements.

A set of Kœnig's forks have been measured with the following results, when corrected to 15° C. :—

A 256 fork measured in May gave	256·309	mean of	19 expts.
Same fork in June and October...	256·310	,,	43 ,,
Another 256 fork	256·306	,,	12 ,,
A 320 fork	320·372	,,	18 ,,
A 384 fork	384·437	,,	24 ,,
A 512 fork	512·451	,,	30 ,,

These forks would be correct at a temperature of about 25° C.

An old fork of Sir William Thomson's, made by Marloye, gave 255·253 instead of 256. This is the mean of ten measurements. This fork would he correct at about 13° C.

Mr. McLeod informs us that the original suggestion of these experiments was made by his colleague, Lieut. Clarke, and that they carried out the experiments together.

Physical Society, *Annual Meeting, February* 8. — Prof. W. G. Adams, President, in the chair.

The following gentlemen were elected as Council and Officers for the ensuing year:—President—Prof. W. G. Adams. Vice-Presidents—Prof. G. C. Foster, Prof. R. B. Clifton, Lord Rayleigh, Dr. Spottiswoode, Sir W. Thomson. Secretaries—Prof. A. W. Reinold and Mr. W. Chandler Roberts. Treasurer—Dr. E. Atkinson. Demonstrator—Prof. F. Guthrie. Other Members of Council.—Capt. W. de W. Abney, Dr. Warren de la Rue, Major E. R. Festing, Prof. Fuller, Dr. Huggins, Prof. A. B. W. Kennedy, Prof. McLeod, the Earl of Rosse, Mr. G. Johnstone Stoney, Dr. Wormell. Honorary Members—Prof. G. R. Kirchoff, Dr. J. Plateau.

Dr. O. J. Lodge read a short paper on a method of calculating the curve of temperature in a rod along which heat is being conducted.

Mr. Shoolbred gave an account of electric lighting, illustrated by diagrams of the most recent magneto and dynamo-electric machines and examples of the lamps in vogue. The only surviving magneto-machine is that of De Meriten's, which is incomparably superior to the older ones of Nollet and Holmes. The dynamo-electric machines described were the continuous-current machines of Siemens, Gramme, Wallace-Farmer, and the alternating-current machines of Wilde, Gramme, and Lontin. Wilde's machine is the first of these, or parent machine, and Lontin's so resembles it that the latter cannot be used in England. In these machines the current from a continuous machine is passed through a second machine, which yields the alternating currents. In Lontin's machine, also, a number of distinct currents are generated in separate circuits, each of which is capable of feeding several lights. There is now one in use on the Western Railway of France which gives three distinct currents,

each of which supplies four distinct lamps, making a total of twelve lights. The American Brush machine was also mentioned. The Dubosq lamp, which was the first regulator, is well adapted for laboratory purposes, but for practical purposes the Serrin is preferable. Rapieff's lamp is used in the *Times* office. The De Mersanne, which was highly spoken of at the Paris Exhibition, moves the carbons by bevelled gearings. The Wallace-Farmer lamp, though durable, is unsteady, perhaps because only inferior gas carbon has yet been used. Jablochkoff's candle was found to be defective from the solid insulator, such as plaster, used between the carbon. This made it very expensive also. Experiments in Paris had shown that whereas Jablochkoff's system cost 10d. per hour per light, the other systems cost only one half of that. In Wilde's candle the solid insulator was dispensed with, air taking its place, the arc always tending to keep at the top of the candle by electro-dynamic repulsion. In the De Meriten's candle three strips of carbon were used, the intermediate one being a stepping-stone to the arc which passes between the two outer ones. Werdermann's and Reynier's so-called incandescent lamps were also shown. Mr. Shoolbred, after alluding to the fact that the upper (positive) carbon takes a crater form, and hence becomes a reflector shedding the light downwards, stated that experiments had proved the line of maximum intensity of light to pass downward at an angle of 60° to the axis of the vertical carbons. By giving the positive carbon a horizontal displacement behind the lower negative one, Mr. Douglas, of the Trinity House, had been able to raise this line till it became horizontal, an advantage in lighthouses. He also pointed out that, whereas in Paris the Jablochkoff waxed for a period short compared to that in which it waned, in London it waxed for longer than it waned, which was of course an improvement; and Mr. Shoolbred suggested that it might be due to the fact that the engine worked at a speed nearer to that of the machine, and that the machine was founded more solidly in London than in Paris.

In the discussion which followed the reading of Mr. Shoolbred's paper,

Mr. Werdermann maintained that it was as easy to produce 500 lights as 10 from the electric light by sub-division, as he hoped soon to show, and stated that the size of the carbons greatly controlled the intensity of the light.

Prof. Ayrton held that the obstacle to the sub-division of the electric light was not an electrical one, but was due to the fact that the amount of light produced by the current is not in direct proportion to the amount of the heat produced.

Prof. Silvanus P. Thompson pointed out that residual magnetism in the cores of the bobbins of dynamo-electric machines lowered their efficiency, and hence short cores, as in the Wallace-Farmer machine, were an improvement.

NOTES.

Biology.

An anonymous writer in a contemporary, raising some interesting questions concerning the sting of the hive bee, declares the queen "stingless"—a novel doctrine.

The question of parthenogenesis among bees is still not absolutely decided, though the observations of Ziernon, confirmed by Sanson, decidedly support the affirmative view.

According to "Les Mondes" a young man died lately from having struck a match upon his finger-nail. A particle of phosphorus got under the nail, occasioning so virulent a burn that death ensued after twenty-seven hours.

It appears, from the "Royal Gazette" of British Guayana, that legislative measures have been taken to protect birds from the ravages of feather-hunters.

Prof. Belucci, in a carefully conducted series of experiments recorded in the "Gazzetta Chimica Italiana," has refuted Clermont's admission of the presence of peroxide of hydrogen in plants.

It is found that the human digestive organs are by no means able to extract all the nitrogenous compounds present in vegetable matters. Hence analytical results throw little light on their true nutritive value.

M. L. Couty has studied the physiological action of matê. He finds that its action is localised in the parts subservient to organic life, and especially the organs relatively least dependent on the nervous centres and specially on the brain. Upon the latter it has no apparent action.—*Comptes Rendus*, lxxxvii., p. 1091.

Dr. J. J. de Lanessan complains strongly of the neglected condition of French institutions for the study of the natural sciences. Thus at Dijon the Professor of Zoology is not even provided with a microscope.

M. Lacerda has laid before the Academy of Sciences certain results concerning the poison of serpents, as obtained from experiments on a *Crotalus* (species not named). He considers that the poison is a ferment, but not of the solid class, containing bodies resembling bacteria, and that it reproduces itself in the blood of animals which die from the bite of the serpent. As an antidote he recommends alcohol, both taken inwardly and injected

under the skin. M. Quatrefages, in presenting the memoir, expressed serious doubts as to the author's determinations.

M. P. Geddes has presented to the Academy of Sciences a memoir on the function of chlorophyll in the green *Planariæ*. These animals, on exposure to sunshine, decompose carbonic acid, and give off bubbles of a gas which was found on analysis to contain from 45 to 55 per cent of oxygen, the residue being nitrogen. A chemical examination of their bodies, after extraction of the chlorophyll, demonstrated the presence of a considerable quantity of ordinary starch.

It is not generally known that there exists a vegetable organism. *Hygrocoris arsenicus*, which is developed in arsenical solutions, It appears as an opalescent cloud suspended in the liquid, and if examined under the microscope appears as a glassy mass scattered over with brilliant points.

M. A. Milne-Edwards has laid before the Academy of Sciences a description of *Blythonomus giganteus*, an isopod measuring 0·23 metre in length and 0·10 in breadth, and which is distinguished from all other crustaceans by the peculiar arrangement of its respiratory apparatus.

M. B. Renault has described a new group of silicified fossil stems of the carboniferous epoch. He establishes the existence of a series of types parallel to that afforded by the Sigillarineæ, but which in certain structural details approach the *Cordaites*.

In a memoir presented to the Academy of Sciences, M. J. de Seynes refers the "chestnut-disease" now raging in the Cevennes to a mycelium analogous to *Zasmidium cellare*, which he finds on all the roots of the trees.

The naturalist E. Beccari has discovered, in the virgin forests of Sumatra, a flower which surpasses all others at once in size, beauty, and perfume. It belongs to the family of the *Amorphophili*, and has received the name of *Titanum*. The diameter of the flower is as much as 83 centimetres. According to "La Lancette Belge" six chests filled with the roots of this plant have just arrived at Genoa.

Prof. Haeckel remarks that the intellectual contrasts existing between the ants and their cattle, the Aphides, are certainly greater than the enormous difference which we recognise between the divine genius of a Goethe or a Shakspeare and the poor animal soul of an Australian negro.

According to Dr. Polli the human organism undergoes, in the course of its existence, a slow oxidation, on the completion of which death ensues. According to his calculation this should happen, accidents excepted, not earlier than the hundredth year. To prolong life a few grammes of a sulphite should be taken every morning in a glass of pure water.

"La Lancette Belge" calls attention to the existence of nu-

merous poisonous fishes on the African coasts, which it fears may be brought into European markets preserved by the application of artificial cold.

According to Prof. Haeckel no expression has given rise to so many errors and misunderstandings as the word "instinct."

Physics.

M. Sergius Kern sends us an account of a very interesting experiment by M. Slouginoff, which is likely to be fruitful in physical research. He takes the two electrodes of a battery, one of which is a thin, plain platinum plate, placed horizontally, and the other is a platinum wire, placed perpendicularly to the plate and nearly in contact with it. A small quantity of water, acidulated with sulphuric acid, is next poured on to the plate. If a current of 8 to 12 elements passes through the apparatus, and the wire is made the cathode, a spot of light is observed on its point. When using 15 elements the light appears, even if the direction of the current is changed. During these phenomena the water is only slightly decomposed, and the needle of a galvanometer, if introduced into the circuit, is only slightly deflected. It was also remarked that the surface of the water under the wire is lowered, forming the shape of a cup. The bubbles of gas arising from the decomposition of the water, in this case, travel constantly round the wire, forming a very pretty figure of 8. This is caused by the movements of the surface of the water, it being alternately repelled or attracted to the wire carrying the current. The gaseous bubbles obey these movements. It was further discovered that during these actions in the apparatus the current was intermittent. It is well known that the mechanical movements in iron or steel bars, during their magnetising by discontinuous currents, may be used as a source of sound. In employing a current of 12 elements, and introducing into it Slouginoff's apparatus, a distinct sound is remarked. If the platinum plate is well polished the water is repelled from the point of the wire equally in all directions and some millimetres from it. The current is thereby interrupted, and the liquid advances to the wire; in this case the liquor will be again repulsed, and so on. Taking advantage of these actions of the current, the apparatus may be employed as a very simple form of an electrical interrupter.

At the Technical Society of St. Petersburg M. Latchinoff recently delivered a lecture on the electric light. He made some experiments with Jablochkoff's condensators, which consist of a set of tin plates placed one on another; the surface of every plate is 0·7 square metre. Between every pair of such plates a piece of silk covered with varnish is introduced. The height of the condensator is about 6 feet. On introducing two condensators into a circuit the intensity of the electric light is doubled.

The lecturer believes the new system of electric lighting devised by M. Rapieff to be a serious opponent of Jablochkoff's process. The chief advantage of the new system is that the luminating point does not change its position, and therefore this system is more suitable for the projection of the electric light at a distance. This advantage will give increase to the use of the electric light for military purposes.

At a recent meeting of the French Academy of Sciences a paper by M. Henri Becquerel, on the "Temporary Magnetic Proportions developed by Induction in certain Specimens of Nickel and Cobalt," was read. The specimens of iron used by M. Becquerel were taken from the same piece of Swedish soft bar iron, of a high degree of purity. By submitting bars of iron and nickel, of the same dimensions and shape, to gradually increasing magnetic influence, it was found that the ratio between the coercitive force acquired by the iron and the nickel is a quantity varying with the intensity of the magnetic force used. This ratio begins by being a minimum of about 0·4; it then increases to 0·75, and again decreases to a still lower minimum of about 0·2. A nickel bar becomes saturated much more quickly than a similar iron bar; the magnetism of the former remains stationary, while that of the latter goes on increasing. Similar results were obtained with a bar of nickel, which was allowed to oscillate over the poles of a powerful magnet. The experiments with the bars of cobalt were similar in their results.

At the same meeting M. Héraud sent in a paper "On a New Voltaic Pile," in which the exciting liquid is a saturated solution of sal-ammoniac, to which one-tenth of liquor ammonia has been added, the depolarising substance being mercuric proto-chloride. With a closed circuit the ammonic chloride is decomposed, the chlorine going to the zinc and the ammonia splitting into ammonia and hydrogen, which reduces the mercury salt to the metallic state, ammonic chloride being once more formed. Each cell is hermetically sealed. After being in action for 227 days, a pile of 9 elements gave an electromotive force equal to 0·73 per cent of the original strength, and at the end of 984 days 0·50 per cent.

Four kinds of camera lucida were brought before the Royal Microscopical Society at their December meeting. Dr. Hoffmann's instrument takes the place of the usual eyepiece. The image of the object is viewed by two reflections; the first by a plate of silvered glass; the second by a plate of transparent glass, through which the paper and pencil are viewed directly. The instrument has the disadvantage of interfering with the employment of the ordinary eyepiece, which has to be removed before the camera can be used, and only permits drawings to be made with the microscope in either vertical or horizontal positions.

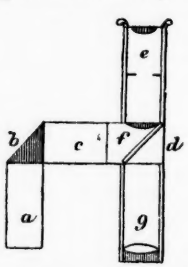
A somewhat complex arrangement of prisms for the same purpose, by M. Pellerin, was also described, but no drawing is

given, and indeed the author's original paper is without illustrations.

Mr. Swift's apparatus is a modification of a well-known form by Nachet. The object is viewed in the usual way, through the eyepiece; the projecting box attached to the cap contains a prism, which reflects the image of the pencil and paper to an inclined tinted plate, where a second reflection directs the image

upwards and renders it visible with the object. This instrument has the advantage of permitting the microscope to be used in any position. A second disc of neutral-tint glass can be interposed when the light from the object requires to be still more subdued to render the pencil point visible.

In Dr. Russell's instrument a tube (*a*) containing a right-angle prism (*b*) fits into the microscope. The image is reflected to a tinted plate (*f*), which again reflects it through the eyepiece (*e*). The tube (*d*) is prolonged downwards, and carries a telescopic

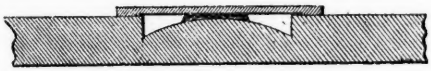


object-glass (*g*), which forms an image of the paper and pencil combined with that from the microscope. The image of the paper requires inversion, either by a reflecting prism (*g*) or suitable lenses placed below the object-glass.

Dr. Hudson, in the discussion which followed the reading of the paper, considered ruled squares placed in the focus of the eyepiece, the contents of which were copied on paper with similar ruling, superior in simplicity and ease of working to any form of camera lucida, especially in the rapid drawing required when living objects were examined.

Mr. Weber, by a simple variation of the hollow slide, has

greatly increased its utility. The cavity, instead of being concave as usual, is ground out, as shown in the cut, having the greatest depth at the edges, the bottom of the cell being convex.



A drop of water is held by capillary attraction between ths cover and the projection; the cover may be fastened or not, at pleasure.

A writer to "Science Gossip" describes a very simple live box, constructed of two ordinary 3×1 slides, placed between two easily fitting flattened brass bands. The slides are kept apart by an india-rubber ring of any convenient thickness, and the whole contrivance kept tight by means of wedges. The contrivance is readily constructed, and applicable to many purposes where a thin glass in front is not required.

The inhabitants of Heilbronn, the native town of the late Dr. Julius Robert Mayer, so renowned for his contributions to the Mechanical Theory of Heat, have resolved to erect a suitable memorial on the spot where he lived, laboured, and died. The following gentlemen have agreed to form an English Mayer-Memorial Committee:—William Spottiswoode, Esq., Pres. R.S.; Sir Joseph Hooker, K.C.S.I., V.-P.R.S.; Prof. Stokes, Sec. R.S.; Prof. Huxley, Sec. R.S.; Prof. Tyndall, F.R.S.; Dr. William Siemens, F.R.S.; Herbert Spencer, Esq.; Sir William Gull, Bart., F.R.S.; Sir James Paget, Bart., F.R.S.; Sir John Lubbock, Bart., F.R.S.; Prof. Henry Smith, F.R.S.; Dr. Debus, F.R.S.; George Busk, Esq., F.R.S.; Dr. Hirst, F.R.S.; Prof. Frankland, F.R.S.; Prof. Dewar, F.R.S.; John F. Moulton, Esq., M.A.; Frederick Pollock, Esq., M.A. Subscriptions exceeding one pound may be sent by cheque to Messrs. Robarts, Lubbock, and Co., 15, Lombard Street. Smaller sums may be sent by post-office order to the Honorary Secretary, T. Archer Hirst, Royal Naval College, Greenwich.

Chemistry and Technology.

The substance of which the much-talked-of Swiss "self-luminous clock-dials" are composed has, according to "The Engineering and Mining Journal," been examined by Prof. Henry Morton, of the Stevens Institute of Technology, Hoboken, and has been ascertained to be the "sulphide of calcium," which is caused to adhere to the dial surface by means of some resinous varnish. This compound has long been known to possess phosphorescent qualities of a high order; but as prepared for this special use its phosphorescence has never before been equalled,

as may be judged from the fact that, after being shut up in a box for five days, it was still visible in total darkness. Prof. Morton expresses the opinion that further advances in this direction may develop discoveries of most surprising character.

We gather from the same journal that Mathey, a Neufchatel chemist, communicates the following facts relating to the same subject. The phosphorescent dials, he states, are usually made of paper or thin card-board, enamelled and covered with an adhesive varnish, upon which is dusted, with a fine sieve, powdered sulphide of barium. The sulphides of strontium and calcium possess the same property; but our authority affirms they lose their phosphorescence more quickly than the barium salt. When the dial has lost its self-luminous qualities they may be restored by an hour's exposure to sunlight, or by burning near it a few inches of magnesium wire.

In a paper presented to the Academy of Sciences, on the "Classification of Colours and the Means of Re-producing Coloured Appearances by Three Special Photographic Proofs," M. C. Gros distinguishes two categories comprised under the word "colours"—lights and pigments. The elementary lights which by their mixture produce all kinds of shades are the green, violet, and orange rays. The elementary pigments which by their mixture produce all kinds of shades are red, yellow, and blue. To obtain directly the elementary tints of rays and of pigments it is sufficient to look through a prism at a white stripe upon a black ground, and at a black stripe upon a white ground. In the first case an orange, green, and violet spectrum is seen; and in the latter case a blue, red, and yellow spectrum. In the former case the orange, green, and violet are elementary rays; and in the latter the red, blue, and yellow are rays combined two and two. The author describes an apparatus which he names the chromometer, and by means of which he produces the photographic effect above mentioned.

A daily paper, speaking of the prizes at the Paris Lottery, uses "carbide of natrium" as a synonym for carbonate of soda. Another newspaper paragraph describes the death of a man who fell into a cistern of "caustic and potash."

According to the "Revue Britannique" ether is consumed as an intoxicant by ladies of rank in England, the grass in Hyde Park being strewed with empty bottles flung from carriage windows.

According to M. Galippe, human hair cut during life has a special odour which remains after the action of potassa or of any other reagent. Hairs which fall off naturally are inodorous, dull, not silky to the touch, and present a special phenomenon of alternating colouration. The hairs of the Chinese, even after the action of potassa, possess an odour of musk, which is intensified at higher temperatures; their section is not round or oval,

but polyhedric, and though black they appear red by transmitted light. The hairs of certain persons retain a peculiar electric condition: if the hand is approached to a bunch which contains some hairs of this kind, they will separate themselves from the remainder and follow the hand.

A curious toxicological case is reported from Hamburg in the "Chemiker Zeitung." The body of a man who died in 1867 was taken up for examination. It was thought necessary to determine arsenic not merely in the corpse in question, but in the soil of the churchyard at different distances from the coffin, and also in the body of another man who had been subsequently buried in the same grave. This latter body was perfectly free from arsenic, which, however, was found in the first corpse in amply fatal quantity (0·24 grm.), whilst in the lid of the coffin and in the adjacent soil very minute quantities were traced. Hence the conclusion was fairly drawn that the man in question had been poisoned with arsenic, and that a portion of the poison had been gradually transferred from his body to the wood of the coffin and the adjacent soil.

In a paper on Poisoning by Mushrooms, in the "Moniteur Scientifique," Mr. J. A. Palmer says that mushrooms may act as a poison in three different manners. They may act as an indigestible matter, which is the case with hard coriaceous species, and may even occur with the edible mushroom when decomposing, as it gives off sulphuretted hydrogen in quantity sufficient to cause vomiting. Or, again, they may be gelatinous or acrid. Many *Boleti*, otherwise innocent, are too gluey to serve as food. Lastly, mushrooms may contain a subtle alkaloid, devoid of smell and taste, as happens in the group of the Amanitæ. This compound is known by the name of Amanitin, and to it the fatal cases of mushroom-poisoning are mostly due. No remedy has yet been found. No immediate effects are produced by this poison; but after eight to fifteen hours the patient experiences stupefaction, nausea, and diarrhœa, followed by delirium and death. Mushrooms containing this poison seem able to communicate it to wholesome species by contact, and it may also be absorbed through the skin. The author was on one occasion seized with alarming symptoms after carrying in his hand some Amanitæ wrapped in paper.

Engineering, Metallurgy, Mineralogy, and Mining.

According to the "Colonies and India" a valuable deposit of graphite, of excellent quality, has recently been made in the province of Wellington, New Zealand.

M. F. Pisani has communicated to the Academy of Sciences the fact that Wagnerite—a fluoriferous phosphate of magnesia, originally met with at Werfen, in Salzburg—is identical with a

mineral subsequently discovered at Bamle, in Norway, and provisionally named Kjerulfine. The latter, however, contains a percentage of lime. M. F. Pisani also finds that the Russian retinite was formerly mistaken for a manganiferous garnet.

A new meteoric mineral, named Daubréelite, has been recently described by Prof. J. Lawrence Smith. In a state of purity it consists of shining black fragments of more or less foliaceous structure, somewhat resembling molybdenite. It is not in the slightest degree attacked by hydrochloric acid, cold or hot, but dissolves slowly in warm nitric acid, without depositing sulphur. Its specific gravity = 5·01; its composition being—sulphur, 42·69; chromium, 35·91; iron, 20·10.

The fifth Report of Progress by the Secretary for Mines of the Geological Survey of Victoria discloses no very salient facts, either in the department of economic geology or of palæontology. An examination has been made of certain elevated basaltic plateaux known as the Bogong and Dargo High Plains, situate on the eastern boundary of the colony—a well-watered district, but from its great elevation subject to frosts even in summer. The region is auriferous, but the distribution of the precious metal is very unequal. Mr. Krause's Report of the Wombat Hill District shows that the auriferous leads are very rich. From 1 mile of this lead 170,000 ozs. of gold have been obtained, equal to a yield of 32 ozs. of gold per lineal foot. In the deep quartz mines of Stawell there is no falling off in gold on descending lower—a fact of considerable scientific interest. The chemist to the Survey, Mr. J. C. Newberry, B.Sc., has discovered phosphates widely distributed in Gippsland, not apparently so concentrated as to furnish an article of commerce, but sufficiently plentiful to impart great fertility to the soil. Some yellowish grey phosphatic soils, hitherto neglected, have been cultivated on Mr. Newberry's recommendation, and have given excellent results.